

# Psychological Bulletin

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# Psychological Bulletin

## THE DOMINANCE-CONTIGUITY THEORY OF THE ACQUISITION OF CLASSICAL CONDITIONING

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In 1930 the writer presented elsewhere (451) a "dominance" theory of "conditioning and related phenomena." Primarily, the theory was an attempt to hypothesize about the neural aspects of conditioning but in a rudimentary way it also purported to be a view of conditioning in general. The view has, all this time, not been fully developed partly because it was felt that more data were needed and partly because further analyses and experimentation led the writer to believe that classical conditioning is not an all-explanatory principle of learning. However, in retrospect it does appear now that "dominance" combined with "contiguity" is a theory most accordant with the facts of the acquisition of classical conditioning, and that while classical conditioning does not explain all learning it does account for a large and significant portion of it. Certainly, classical conditioning contains, to date, a greater abundance of obtained data (some 1,500 separate experiments), a greater portion of reasonably verified data, and an objectively more manipulable and less equivocal set of variables than any other variety of learning. And one may indeed further argue that in some fashion, even if in varying degrees, principles of classical conditioning enter all learning—so that no matter what one's theoretical biases may be, no understanding of any learning is com-

plete without a prior analysis and conceptualization of the basic mechanisms of classical conditioning.

The purpose of the present article will thus be to formulate anew a "dominance-contiguity" theory of the acquisition of classical conditioning—to formulate it in the light of both (a) old research that has been newly treated and (b) the large amount of new research—and thought—that has gone into this field in the last 25 years. The formulation is, it will be noted, confined to the *acquisition* of conditioning. The writer's views on classical CR extinction and partly also on classical CR generalization-differentiation have been presented elsewhere (456, 468; 461) and while an integration of those views with the present ones would be desirable, it cannot, for lack of space, be undertaken here. Again, as indicated, the formulation pertains only to classical conditioning. The acquisition of reinforcement learning (or operant or instrumental conditioning) and of relational-configural learning manifests, in the writer's opinion, characteristics that are not wholly reducible to, or deducible from, characteristics of classical conditioning (nor wholly reducible to, or deducible from, each other's characteristics); and hence its theoretical codification is quite a separate and more difficult (though not unrelated) task—rather, two separate tasks—which is again

not within the scope of the present topic. Finally, the writer regrets that the neural supports of the present theory, unlike its behavioral bases, are still in the nature of conceptual inferables (or an "inter-phenomenon" with only "epistemic correlations"—to borrow terms from current philosophy). It is hoped, however, that these inferables, too, will become observables: a realm of perpetually unredeemed inferables is not the writer's ideal for psychology.

#### THE EMPIRICAL BASE OF THE THEORY

The empirical base of the dominance-contiguity theory of the acquisition of classical conditioning rests upon seven separate but related empirical parameters. Four of these parameters have been derived from experiments on salivary conditionings of dogs in Russian laboratories—Pavlovian and post-Pavlovian (300 separate experiments, designated in bibliography by letters *i*, *m*, *im*, and *d*)—whereas the remaining three stem from a reanalysis of data of the writer's studies of salivary conditioning in adult human subjects (457, 458, 459, 460, 462, 464, 465, 469, 471, 472; 463). There is obviously an advantage in basing a CR theory, at least at the outset, upon the conditionability of one response when the evidence for such conditionability is not only amply extensive in itself but also constitutes about one half of all the evidence in the area. Again, there is obviously also some added value in utilizing for the theory evidence from both animal and human experiments. However, it should be stressed here that in this particular case the added value lies not, as might be thought, in the special differences that obtain between animal and human conditioning with the same methodology—

differences which the writer, in recent years, has sought to minimize through controlling rigorously the attitudes of his subjects—but rather in the special methodologies which each type of experimentation offers for the probing of general CR parameters. As will be seen later, the three general CR parameters uncovered by the writer's special methodology with human subjects could hardly, if at all, be duplicated in animal studies—although these parameters, obtained from only several studies, could hardly be accorded the same definitive status as the four ingrained in several hundred Russian experiments.

A preliminary word should also be said now about the nature and treatment of the Russian data. A complete critical and statistical analysis of all Russian work on CR acquisition—and all CR experiments obviously include some data on acquisition—is an enormous task, not only because of the noted volume of 1,500 separate studies but more so because Russian CR reports contain, as a rule, only raw data without the benefit of even the simplest measures of central tendencies and variabilities. However, since results of only salivary conditioning in dogs were of primary concern here, the number of "to-be-treated" experiments could be reduced to about 700. Moreover, a direct examination of the reports of 618 of the estimated 700 experiments disclosed that only about one half of them contained significant quantitative data on the desired parameters, which suggested that but very little information would be lost if actual statistical treatments—primarily *Fs*, sign-tests, and *rs*—were applied to only an even number of 300 reports (all the 618 examined reports are listed in the bibliography and all the 300 treated reports are designated,

as was already noted, by appropriate letters). And again, while the collation of the results of different experiments presented some special problems, the task was greatly facilitated by the fact that the Russian studies are very uniform in design and control of variables. Still, it should be stated that the empirical base of the present article is in a sense considerably wider than that of all of the reviews of Russian experimental CR literature—ten in number (452, 453, 454, 455, 456, 461, 467, 468, 470, 473)—which the writer has published until now. And, incidentally, it may be worth noting that the *stated* findings of the Russian experiments were borne out in 71% of the cases. In the remaining 29, they were not; that is, their data could not be made to disprove the null hypothesis at the 5% level of confidence by any conventional statistical technique.

### *The Seven Parameters of the Acquisition of Classical Salivary Conditioning*

1. *CS-intensity.* Even a very cursory examination of the protocols of the pertinent reports of Russian experiments (161 reports, designated in the bibliography by letters *i* or *im*) quickly reveals that significant variations in CS-intensity—whether expressed in physical units as decibels, international candles, temperature degrees, distances from source of stimulation, etc.; or merely stated by experimenters as "loud," "medium," "weak" sounds, "strong," "medium," "faint" odors, "bright," "medium," "dull" lights, etc., often also "very loud," "very strong," "very bright," "very weak," "very faint," "very dull," etc.—are very significant determiners of CR efficacy as manifested in speed of CR formation and in the magnitudes and latencies of resulting CRs. At first glance, the

relationship appears to be all direct: the higher the CS-intensity, the faster (fewer trials) the conditioning and the higher the magnitudes and the shorter the latencies of the CRs. However, a closer scrutiny shows that in the upper portions of the CS-intensity continua, inverse relationships prevail, that beyond certain points in these upper portions the louder the sounds or the brighter the lights or the higher (or lower) the thermal stimuli, for instance, the slower and the lesser the conditioning.<sup>1</sup> True, the fact that the Russians seldom evaluate psychophysically the CSs in their studies does not permit a ready cross-modal or even intramodal but cross-dimensional unified generalization on the specific relation of CR-efficacy to CS-intensity. Yet, the numerous intradimensional CS-intensity curves of conditionability which the writer plotted from the Russian data are almost uniform in

<sup>1</sup> While results of direct variations of CR magnitudes and latencies with CS intensities are complicated, as correctly pointed out by Hilgard and Marquis (169), by the possibility of their being a non-CR S-R rather than a true C-R function, the same cannot obviously be said about such variations with speeds of CR formation. Moreover, the decline in CR-efficacy with high intensities of CSs is evidence also against a mere S-R conception of magnitude and latency variations. Again Grant and Schneider's (155, 156) finding that resistance to extinction did not vary with CS-intensity in human eyelid conditioning bears but little upon the parameter under discussion, both because the parameters and mechanism of CR extinction differ from those of CR acquisition (456, 468) and because, as Grant and Schneider would no doubt admit themselves, of the possibility of attitudinal modifications (extinction in particular is susceptible to such modifications). The fact is that a postulation of a CS-intensity parameter for Pavlov's results is truly hard to avoid: it is evident not only in the more than a dozen experiments specially devoted to its investigations but also in the many scores of others in which CS-intensity data are available for comparison.

their general characteristic of slow ascending gradients followed by rapid descending ones, and in suggesting that the conditionability of CSs possesses not only its own LT (lower threshold)—which has long been suspected to lie below the LT (RL) of psychophysical sensitivity—but also—what is generally not known—its own UT (upper threshold), lying considerably below the UT (TL) of psychophysical sensitivity.

More than this, the presumed painful quality of sensory stimuli at high intensities could hardly be construed as the determining factor of their poorer conditionability. For first, the reversals in conditionability begin at points below those judged by human subjects as painful, and the general behavior of the dogs in such experiments does not appear to indicate algesia, either. Second, mildly painful stimuli—and none of the sensory CSs used could be more than mildly painful—such as mild electric shocks are readily conditionable, that is, become readily transformed into CSs with food as URs (105, 119, 293, 645, 646). And third, below a certain point, the more painful (mildly painful) a CS is, the greater is its CR-efficacy, as evidenced by the fact that the conditionability curves of electric shocks as CSs differ from similar curves of sensory CSs only in that in the shock curves the CR-reversals occur earlier in the series (*ibid.*). In short, the reversed conditionability trend must in the main be attributed to the CS-intensities per se irrespective of possible accompanying pain and general antagonism, even though the latter may well be contributing factors in some cases.<sup>2</sup>

<sup>2</sup> Conditionability seems also to vary directly with frequencies of tactile CSs as well as, to some extent, with frequencies of metronomes as CSs and even with vibration

2. *UR-magnitude.* The evidence for UR-magnitude as a parameter of CR-efficacy is in a large way even more clear-cut than that of CS-intensity. Russian CR reports quite often record the amount of unconditioned salivation secreted by their subjects in each training and testing trial in the course of 30 seconds (reports designated in bibliography by letters *m* or *im*)<sup>3</sup> thus offering ready comparisons of CR-efficacy with UR-magnitude. Moreover, the parameter itself is simpler and more uniform. Except for some special conditions which will be accounted for in a later section, the parameter is wholly monotonic: the greater the magnitude of the UR, the faster the conditioning and the greater the mag-

frequencies of sound CSs in general—all of which, certainly the frequencies of tactile CSs, might be attributed to an intensity or intensity-like factor. Similarly, the undoubtedly CR superiority of complex or multiple CSs might be classed as a case of CS-intensity, although here Guthrie (165) and Estes (99) would be well justified in appealing to the concept of CS-complexity, that is, the availability of greater numbers of "unit-conditioners." On the other hand, while the conditionability differences among *kinds* of CSs—inter- and intramodal—which the Russians repeatedly report, might relate to differences in psychophysical values among the CSs as commonly used in the laboratories, the possibility of a pure nonintensive qualitative CS factor in CR-efficacy must by no means be excluded. Auditory stimuli in particular appear to possess special conditioning powers.

<sup>3</sup> Three separate measures of salivary UR-magnitude are possible: (a) total amount of UR salivation in a training or a reinforcement trial, (b) UR salivation per unit of food (or HCl) administered in a trial, and (c) UR salivation per unit of time. However, Russian *Es* report only occasionally total unconditioned salivation and, moreover, in a large number of cases the unit of food or HCl is constant; hence only the third, the per-unit-of-time measure was available for large-scale computations, and it is the one which yielded the most significant results. The entire matter is presented in detail elsewhere (472).

nitude of the CR. There is no reversal of CR-efficacy and no UT of conditionability with respect to UR-magnitude. On the other hand, the host of data clearly show here another limiting feature of conditioning, namely, that conditioning is not readily or not at all effected when the UR is comparatively small in magnitude. Or, in other words, the LT of conditionability with respect to UR-magnitude, unlike the corresponding LT with respect to CS-intensity, is by all tokens considerably above its own reactivity (occurrence).

Again, as is well known, there are also several Russian (and other) studies which show that CR speed and magnitude vary also directly with US-magnitude (amount of feeding or HCl administration; 45, 52, 110, 114, 193, 286) and duration of food deprivation (conventionally equated with drive strength); (38, 134, 480, 667, etc.). However, these studies are comparatively few in number and the correlations of their results with CR-efficacy are by and large not as clear-cut as those of UR-magnitude. *Moreover—and this is the crucial consideration—variations in US-magnitude (or US-intensity) and in duration of food deprivation are both well reflected in the variations of UR-magnitude, and thus are of any specific value only when information on the latter is not available.* Otherwise, they are operationally superfluous. What correlates with variations in CR-efficacy are variations in UR-magnitude, and it seems to matter little whether these magnitude variations are produced by differences in US-magnitudes, differences in the duration of food deprivation, administration of drugs, bodily injuries, or even mere individual differences between animals, or something else. Hence, a concept like drive strength

is, as far as classical conditioning is concerned, of little heuristic and predictive value (the situation is quite different in operant conditioning). And if there is no value in drive strength, there is no meaning in drive reduction.<sup>4</sup>

3. *UR-magnitude CS-intensity ratio.* The two preceding parameters have each been obtained, as might have been noticed, independently; that is, the conditionabilities of different CS-intensities or of different UR-magnitudes were compared without regard to the values of the other accompanying variable involved in the CR situation. Such an analysis, however, obviously masks the probability that CR-efficacy is not just a function of the absolute values of each variable but also a function—or pos-

<sup>4</sup> Drive reduction could be made to be used with specific significance in classical conditioning only in a very special sense that is quite different from the conventional one, namely, as a substitute for response reduction or, possibly as an organismic state involving (or determining) response reduction, either of which may itself become a CR datum, that is, function as a UR in a CR situation. Thus, when a CS in Pavlov's or in the writer's studies is applied at the end of a meal, the CS comes to produce response reduction rather than response incrementation (or response induction, to borrow the last word from Mowrer), which obviously means that the response reduction or, possibly, the organismic state involving it, became itself conditioned as a UR. The qualification "possibly" is used because evidence for the classical conditioning of organismic states as such is available only at the human level (466). And incidentally it might be added that what has been said here about drive reduction applies also generally to drive induction—that is, it, too, should be used in classical conditioning only as a substitute for response induction or, possibly, as a conditionable organismic state involving (or determining) such induction, and not as a condition, necessary or sufficient, for conditioning—except of course that drive induction, unlike drive reduction, parallels normal and not exceptional CR situations.

sibly even only a function—of the values of each variable relative to those of the other. Consequently, it was imperative to break up the analysis into comparisons of the conditionability of comparable ascending or descending series of values of one variable at different fixed values of the other. And the separate comparisons did indeed yield very instructive results. First, it became clear that if the series were fairly complete, the CR-efficacy curves almost always included an ascending gradient followed by a descending one (it will be remembered that when the values of the other variable were not taken into account, the ascending-descending characteristic was common only in the CS-intensity curves while the curves of UR-magnitude were mainly monotonic). Second, it was found that the higher the fixed values of the other variable, the further along in the series did the ascending-descending point of reversal occur. Third, it was generally true that the higher the fixed values of the other variable, the higher was the CR-efficacy of the series.

Or, compressed in mathematical symbols, we might say that if  $a$  is a fixed value of one variable in the CR situation and  $b_1, b_2, b_3, \dots, b_n$  is a fairly complete series of values of the other, then the values of  $c$ , the CR-efficacy of the  $b_1 \dots b_n$  series, will increase up to  $b_{n-s}$  and decrease afterwards, while, in addition,  $x$  will vary inversely and  $c$  will vary directly with  $a$ .

Thus, the "empirics" of the acquisition of classical conditioning may so far be summed up in four determinative statements: (a) The acquisition of classical conditioning is essentially limited by two thresholds: an upper threshold of CS-intensity—the CS must not be too intense—and a lower

threshold of UR-magnitude—the UR must not be too small. (b) Within the thresholds, the efficacy of the acquisition varies directly with absolute values of UR-magnitudes and at first directly and then inversely with absolute values of CS-intensities (the inverse relationship occurring only at the upper segments of the CS-continuum). (c) The efficacy of the acquisition varies also—and probably more significantly—with the ratios of the UR-CS values, there being by all signs an optimum UR-CS ratio (or ratios) at which classical CR acquisition is most efficacious while both below-optimal and above-optimal ratios manifest decreasing effectiveness. (d) The gradient of decreasing efficacies of above-optimal ratios appears in general to be considerably steeper than the corresponding below-optimal gradient.

4. *CS-US and US-CS delays.* A complete analysis of this parameter is in a way handicapped by the fact that in alimentary conditioning the Pavlov laboratories seldom use CS-US or US-CS delays of less than one second, thus somewhat truncating their data at one end. On the other hand, however, the Russians have in general always been greatly interested in delayed conditioning because of the light that it supposedly sheds on the so-called cortical mechanisms of delayed inhibition and of the duration of inhibition and excitation; and they have, therefore, performed a good number of experiments with CS-US delays ranging from one second to several minutes which provide ample material for curve-analyses of the effects of such delays on CR-efficacy (35 experiments, designated in bibliography by letter *d*). Broadly, these curves appear to be of two main kinds: (a) those which manifest a steep rise in CR-efficacy up to delays

of a few seconds and then a gradual decline as the delays are lengthened, and (b) those in which only gradual declines in CR-efficacy with increases in delays are revealed. The first kind of curve is common when the CSs used in the conditioning are of high or of medium intensity and the UR-magnitudes are comparatively low, whereas the second kind of curve predominates when CS-intensities are rather low and the magnitudes of the URs are of high or medium magnitudes. Moreover, while the slopes of the curves of delays in trace CRs—that is, when the CS is terminated before the activation of the UR—are, as might be expected, considerably steeper than those of the curves of delays in delayed CRs proper—that is, when the CS continues until after the activation of the UR—the general shape of the curves in the two types of CRs is very much alike.<sup>5</sup>

Now, while on the face of it the curve-characteristics of this parameter appear to be quite different from, and indeed the reverse of, those of the preceding parameters—steep ascents followed by gradual descents vs. gradual ascents followed by steep descents, or only gradual descents vs. only gradual ascents—a simple assumption suggests that in reality this parameter fully corroborates the CR-efficacy relationships so far discussed. The assumption is not an unfamiliar one and is well implied in the writings of Pavlov, Hull, and others—namely, that the lengthening of an interval between the onset of a CS and a US

is equivalent to the weakening of the intensity of the CS. For then the conditionability curves of lengthening CS-US delays are actually conditionability curves of *decreasing* series of CS-intensities and would be expected to yield steep ascents followed by gradual descents, inasmuch as the conditionability curves of *increasing* series of CS-intensities have been shown to produce gradual ascents followed by steep descents. And again, the CR-curves of lengthening CS-US intervals with only gradual descents might be viewed as curves of incomplete series of decreasing CS-intensities in which the upper above-optimal higher intensities were lacking, a view supported by the observation that such curves are common only when the CS-intensities are low. Indeed, it may well be contended that, as far as basic mechanisms of the acquisition of classical conditioning are concerned, this fourth temporal parameter is merely a special case of the first and third parameters, the parameters of CS-intensities and ratios of CS-intensities.

Finally, a word should be said about the relation of CR-efficacy to US-CS delays, or to lengthening delays in backward conditioning. The existence of true backward conditioning has, as is known, been denied by most CR workers in this country and its alleged manifestations attributed to pseudo-conditioning. However, a number of recent Russian experiments, reviewed in this Journal by the writer (470), clearly show that such conditioning is a genuine CR phenomenon and occurs with a fair degree of stability under certain special conditions. And as so far determined, the main special conditions are either (a) a UR that is rather low in magnitude or (b) a US-CS interval of considerable length,

<sup>5</sup> Classical alimentary conditioning experiments in dogs do by no means support the generalization, based largely upon aversive conditioning in human subjects, that CS-US delays of 0.5 second are invariably optimal. First, the optima are here quite variable, varying, as we have seen, with CS-intensities and UR-magnitudes; and, second, they are generally longer than one second.

10-15 seconds (*ibid.*), which suggests that the lengthening of US-CS intervals is equivalent to the weakening or decreasing UR-magnitudes (just as lengthening CS-US intervals is equivalent to weakening CS-intensities). Hence, this phase of the present parameter may also be regarded as no more than a special case of parameters two and three, the relation of CR-efficacy to UR-magnitudes and to UR-magnitude relative to CS-intensities. And the contention is further supported by the consideration that the curve of CR-efficacy plotted against lengthening US-CS delays tends to be bell-shaped (the data are meager here), which, it will be remembered (*supra*, p. 6), is also the shape of the curve of CR-efficacy with low-value UR magnitudes ( $x$ , the point of reversal in CR-efficacy varies inversely with  $a$ , the value of one of the two variables in the CR situation).

5. *CS-attensity.* This Titchenerian term refers here to the attention value of a CS as determined by an objective test of CS recognition. In a number of the writer's studies (*op. cit.*), the CSs were multiple-item visual sensory or visual verbal units—series of miniature lights of different colors and shapes, and series of familiar and/or unfamiliar words—and it was an easy matter to mix these CS-units with corresponding nonconditioned stimulus-units and obtain recognition scores as indices of CS-attensity.\*

\* The subjects' instructions in these studies were that "the purpose of the experiment is to study the effects of eye-fatigue upon digestions," and no sign was given that any kind of memory test was involved. Hence, it seemed legitimate to assume that the recognition scores reflected much more the amount of attention that the subjects paid to the CS tasks at each trial than the efficacy of their retentive capacities, particularly since the scores ranged from 5 to 98 per cent with

When an attempt was now made to plot these attensity values against values of CR-efficacy, it was soon discovered that the correlations between the two would be curvilinear or, more specifically, that the low and medium attensity values would yield positive correlations and the high attensity values negative ones. Accordingly, separate computations were made for attensity values below and above 75 per cent of recognition. And the results were instructive indeed. Below 75 per cent, the mean correlation between the attensity and the CR-efficacy scores was .51 while above that point it was -.41 (correlations were calculated separately for each CS-unit and then averaged by means of  $z$ -transformations), thus demonstrating not only the existence of an attention parameter in classical conditioning but also the fact that the basic nature of this CS-attensity parameter is much like that of CS-intensity; i.e., ascendant gradients followed by descending ones. Moreover, inasmuch as the results showed also correlations between CR-efficacy and the lengths (number of items)—or what might be called the intensities—of the CS-units (cf. Footnote 2), and inasmuch as these lengths or CS-intensities correlated further, as might be suspected, with the attensity values—it seemed legitimate to compute multiple correlations of the CR-efficacies with respect to the combined effects of CS-attensity and what has been called CS-intensity. The multiple correlations were .58 for

a mean of 47, while they no doubt would have approached 100 per cent in all cases if the subjects knew that they were to reproduce the CS's later (15-30 items, 20-40 trials). Again, it might also be worth noting that attensity refers here of course to the CS tasks and not to the conditioning itself; the subjects did not know that they were being conditioned.

attensity values below 75 per cent and their corresponding intensity values, and  $-.43$  for attensity values above 75 per cent and their intensity scores.

6. *UR-affectivity.* In most of the writer's studies, the subjects are asked to rate on a scale of 5 the degree of their enjoyment of the food consumed. And these ratings correlated with CR-efficacy even more significantly than the attensity indices. With an  $N$  of 150, the zero-order correlation between affectivity ratings and values of CR-efficacy was found to be  $.54$ . However, since the ratings also correlated substantially,  $r = .62$ , with UR-magnitudes and the magnitudes correlated in turn with CR-efficacy to the extent of  $.56$ , the net partial correlation (correlations were linear) of UR-affectivity with CR-efficacy was  $.30$  which is still evidence for a specific parameter of UR-affectivity in classical conditioning. The multiple correlation of CR-efficacy with respect to the combined effects of UR-affectivity and UR-magnitude was  $.61$ .

7. *UR-affectivity CS-attensity ratio.* The finding that the CR effects of variations in UR-affectivity and CS-attensity in the writer's studies were characteristically similar to those of variations in UR-magnitude and CS-intensity in all the Russian experiments suggested, among other things, that here, too, conditionability might be significantly related to ratios of values in the two variables. Moreover, it was in general desired to subject the writer's data to a more detailed analysis and to use a less global technique than coefficients of correlation. Consequently, conditionabilities were determined separately for each of the five affectivity ratings at all the differing attensity values with which they were paired. The results

with affectivity ratings of 1 were too few and yielded too little conditioning to be considered here. However, with each of the four main ratings—where results were abundant—optimum conditioning almost invariably occurred at medium and high-medium attensity values, while "no significant conditioning" predominated at both low-attensity and high-attensity scores. Moreover, the attensity scores of both the four optima and the four low and four high "no significant" conditioning—the first of which might be called the lower and the second the upper threshold—were invariably higher when the affectivity ratings were higher and lower when the ratings were lower. With affectivity ratings of 5, the attensity values for optimum conditioning and for low and high "no significant" conditioning were, for instance, 71.3, 29.4, and 88.4 per cent—while for affectivity ratings of 2, the respective figures were: 54.2, 8.4, and 68.2. Again, the CR-efficacy values of the four optima of conditioning were, in their turn, higher when the affectivity ratings were higher and lower when the ratings were lower. In terms of standard CR scores, the mean optima of conditioning for affectivity ratings of 5, 4, 3, and 2 were: 19.4, 15.7, 13.1, and 9.1. (The differences between affectivity ratings of 5 and 2 were significant at the 5% level of confidence or better in all cases. The differences between ratings 4 and 3 were not significant in any of the cases, while the differences between ratings 4 and 2 and between 5 and 3 were significant in some cases and insignificant in others.)

Thus, the hypothesis in the first sentence of the preceding paragraph was in the main fully corroborated. The two "psycho-central" CR parameters uncovered in the writer's stud-

ies—CS-attensity and UR-affectivity—interacted with each other in a manner that is quite similar to the interaction of the two "physico-peripheral" parameters—CS-intensity and UR-magnitude—in the Russian experiments. Moreover, it should be added here that the analysis of the relation of the "psycho-central" CR parameters to each other brought out a significant negative correlation of  $-.48 \pm .1$  between CS-attensity and UR-affectivity when the attensity scores are above 75%, which plainly corresponds with the repeated finding in Pavlov's laboratories that CSs of very high intensities reduce UR-magnitudes (341, 342) and, to some extent, also with related evidence that in human subjects CS-sensitivity may be decreased when UR-magnitudes are very high.

#### THE THEORY PROPER

To a considerable extent, what has been said so far might well be considered as already constituting a "theory" of conditioning or even of learning, at least as far as currently stressed issues are concerned. Thus, there has been here the repeated assertion that conditioning may be effected only within certain limits of CS and UR characteristics, which obviously means that the mere occurrence of a UR does not in and by itself ensure the conditioning of all—or even of any—CSs that occur with it. Or, in other words, contiguity is a necessary but not a sufficient condition for the acquisition of conditioning. Again, it was maintained that in classical conditioning, drive strength is an operationally useful parameter only when UR magnitude data are unavailable, and that drive reduction is of specific significance only in a very special sense that is quite unrelated to the conventional one,

namely, as a substitute for UR reduction when a CS is applied while a UR is being reduced, as at the end of a meal, and the drive reduction (or UR reduction) is thus a datum rather than a condition of classical conditioning. Hence, reinforcement in the sense of drive reduction is neither a sufficient nor a necessary condition for classical conditioning, and motivation (drive strength) is only a general energizer or an indirect and more or less approximative *UR indicator*, rather than a direct and independent *CR determiner*. Moreover, it was also contended throughout the preceding section that conditioning is significantly, and in human subjects almost equipotentially, determined by both central factors—CS-attensity and UR-affectivity—and peripheral factors—CS-intensity and UR-magnitude—which no doubt may irk both the centralists and the peripheralists among learning theorists but which, nonetheless, is a fairly distinct view in the area. And finally there was the unmistakably distinct and crucial concept of specific thresholds of conditionability and of optimal UR/CS ratios of CR efficacy.

On the other hand, it is obvious that from a formal standpoint what has been presented so far could be better described as a set of empirical generalizations, or, more formally, of "low-level" functional relationships, rather than a theory in the conventional sense. No intervening variables or hypothetical constructs or any "higher-level" hypotheses were included—not even an existential hypothesis (Feigl, 120) such as "proprioceptive CSs" ("movement-produced stimuli"). Without exception, the obtained functional relationships were between directly and objectively observable and measurable variations in stimuli and responses involved in

the CR situation. And the only unresolved hypothesis in all of the presented material, the assumption that the correlation between CR-efficacy and CS-intensity proceeds along one general psychophysical dimension irrespective of special sense modalities and qualities, is clearly low-level and inductive in character and would indeed not have existed if the Russians had evaluated their CSs in psychophysical terms (and will no doubt cease to exist in the near future). Again, while attensity scores and affectivity ratings have been labeled here "psychocentral," the label need not, of course, be regarded as more than a nominal one and the scores and ratings as more than mere verbal responses that have somehow become attached to the CSs and the URs, thus satisfying even the purest of peripheralists. And yet, it certainly would be hard, on the face of it, to assert that the present low-level *inductive* account of classical conditioning lacks any of the virtues of the higher level *deductive* theories with respect to testability, fertility, predictivity, and "systematic power" (Hempel, 168) in general; or to argue that a fuller development of the account in the present form—extension to responses other than salivation, determination of exact CR thresholds and exact UR/CS optima—would in any way be aided by the special features of "construction" theories. Does it, then, mean that "theories of learning are unnecessary" (517), or at least that classical conditioning need not resort to anything beyond its own level of direct observables to be fully systematized?

The writer's answer to the last question is, nonetheless, a definite "No." The account of classical CR acquisition as so far presented ap-

pears to him wanting in generality, integrability, economy, meaning, and "explanatoriness" in general, which an appropriate supplementary integration at a higher level of "unobservable" underlying mechanisms could impart to it. Moreover, it is the writer's contention that an appropriate "explanatory" integration of the present account is well achievable through three simple and somewhat obvious assumptions about the nervous system or, more specifically, about the CS-initiated and the US-initiated neural events in the conditioning, viz.: (a) that the two neural events initiated by the CS and the US are the immediate antecedents or "effectors" of the CR acquisition; (b) that the values of the CS-initiated neural event<sup>7</sup> (whatever the exact nature of the event and the values) vary directly with the values of CS-intensity and CS-attensity and the values of the US-initiated event vary directly with the values of UR magnitude and UR-affectivity; and (c) that the two neural events have common characteristics and common points of interaction. For, if these assumptions are granted—and it is hard to think of a modern psychology that would not grant them—the parametric data become readily translatable into neural terms, and the resulting translation becomes a systematic means of realizing the mentioned desiderata. For example, the obtained correlations of CR-efficacy with CS-intensity and CS-attensity may well be said merely to reflect or symptomatize the correlation of this efficacy with the CS-initi-

<sup>7</sup> Neuroendocrinic or intraorganismic event might of course be substituted for neural event, the first for obvious reasons and the second to allow for preneural conditioning in lower invertebrates and possible nonneural conditioning of some vegetative functions.

ated neural event while the correlations between CR-efficacy and UR-magnitude and UR-affectivity reflect or symptomatize the correlations between this efficacy and the US-initiated neural event, *thus reducing four parameters to two*. Then, the two CS and the two UR correlations with CR-efficacy may further be said to reflect or symptomatize the correlations of separate but overlapping and supplementary aspects of their corresponding neural events with CR-efficacy, a conclusion that links a consideration of the empirical size of the parametric zero-order and partial, and multiple correlations with a logical analysis, in neural terms, of the relation of CS-intensity to CS-attensity and of UR magnitude to UR-affectivity. And certainly, the two parameters of optimal UR/CS ratios of conditionability and the finding of UR/CS interferences and inverse relationships are much more meaningful when thought of as results of interacting neural events than as mere interrelations between interchangeable values of discrete behavioral variables.

Now, consider more specifically the nature of the relation of CS-intensity and of CS-attensity to a CS-initiated neural event, and a moment's thought suggests that in the first case the relation is of a whole to a part and in the second case of a part to a whole. CS-intensity (and CSs in general) obviously bounds the total amount of energy available to a CS-initiated neural event of which normally only a part is utilized by, or transformed into, it, while there is no doubt that CS-attensity reproduces only a part of the utilized or transformed CS energy, namely, the part that has come to be consciously recognized or verbally identified. In other words, the three variables might be repre-

sented by three concentric areas in which both the larger and directly observable area of CS-intensity and the smaller and directly observable area of CS-attensity correlate, each in its own way, with the middle area of the CS-initiated neural event. Or, if  $A$  denotes CS-intensity and  $a$  CS-attensity, then the CS-initiated neural event equals  $Ax^{-1}$  and  $ax_1$ ,  $A$  equals  $axx_1$  and  $a$  equals  $Ax^{-1}x_1^{-1}$ , where  $A$  and  $a$  are known quantities and  $x$  and  $x_1$  are unknown neural coefficients greater than unity. Likewise, we may come to regard UR-affectivity as a part of UR-magnitude and UR-magnitude as a part of the US-initiated neural event. Or, if  $b$  denotes UR-affectivity and  $B$  UR magnitude, the US-initiated neural event equals  $byy_1$  and  $By_1$ ,  $B$  equals  $by_1$  and  $b$  equals  $By_1^{-1}$ , where  $b$  and  $B$  are known quantities and  $y$  and  $y_1$  are unknown neural coefficients greater than unity. And it seems worth reproducing here the actual correlations between CS-intensity and CS-attensity and between UR-magnitude and UR-affectivity, as well as the "beta weights" of each of the four parameters and their separate and combined "accountable variance" with respect to CR-efficacy. The correlations were .46 and .62 (disregarding correlations with attensity scores greater than 75%), while the computed "beta weights" are: CR-efficacy = .37 CS-attensity + .31 CS-intensity + .31 UR-affectivity + .37 UR-magnitude, which, multiplied by respective zero-order correlations with CR-efficacy, yield respective "accountable variances" of 19 + 15 + 17 + 21% or a total of 72% and an  $R$  of .65. The fact that only 28% of the CR-efficacy could not be accounted for by the four parameters is indeed striking.

Again, continuing the analysis into the crux of the present account, the

nature and the characteristics of the interaction between the CS and the UR variables, three phases seem to need specific translation into neural terms. First, there is the unmistakable finding that the lower limits of conditionability are considerably higher in the UR than in the CS continua, which in conjunction with the fact that typical USs produce in general greater neural changes than CSs, clearly suggests the neural generalization that for conditioning to be effected the value of the US-initiated neural event must be considerably greater than that of CS-initiated neural event, that is, the former must "dominate" the latter.<sup>8</sup> Second, there is the evidence that the optima and the means of CR-efficacy are higher when the values of either the CS or the UR variables are higher and lower when these values are lower; or, in neural terms, that conditionability varies directly with the values of both the US-initiated and the CS-initiated neural events. Third come the decreases in CR-efficacy when UR/CS ratios exceed their optima, as well as the negative correlations between UR-affectivity and CS-attensity when CS-attensity is high and the decreases in UR-magnitudes when CS-intensity is very high, to indicate that beyond certain points the US- and the CS-initiated neural events interfere in some manner with each other and that by all signs this interference varies directly with the interfering variable and inversely with the interfered one.

<sup>8</sup> In the earlier article (451), "dominance" was derived primarily from the finding that the conditioning of an electric shock as a CS to a food UR becomes reversed—that is, the food becomes a CS and the shock a UR—when the shock is increased in intensity and/or the food UR is decreased in magnitude, which may well be construed as additional evidence for the concept.

Or, to sum up in somewhat different and paradigmatic terms, the writer's position on "facts" vs. "theory" in conditioning and learning lies somewhere between that of Skinner (517) and Hull (170, 171), and his view on neural action and conditioning somewhere between that of Pavlov (341, 342) and of Bergmann (62) and Spence (553). Like Skinner, the writer holds that the primary task of the psychology of learning is to discover low-level (directly observable) facts and to array them into low-level functional relationships (parameters, laws) with a use of low-level concepts, and that this task may be—and often is—hampered and misled by "transcendent" theories and concepts that are only loosely and fragmentarily anchored to empirical bases. Hull's system, overloaded with "conditional" logic as against "unconditional" psychology and with "implications" as against "replications," has always appeared to the writer to be particularly vulnerable in this respect, and he is not surprised that its pillars-postulates need so much continual propping. (A special, although indeed an almost unavoidable, weakness of Hull's system is his unquestioning acceptance of the facts of classical conditioning as reported by Pavlov—facts which, it must be remembered, are the fulcrum of most of his postulates, his own specifics of motivation and effect and the mathematico-logical methodology notwithstanding.) On the other hand, the writer would also contend that *after* a fair empirical systematization of an area of low-level facts has been achieved—and this is considerably easier in classical than in operant conditioning both because the former has more facts and because in the latter the facts are not at their lowest com-

mon denominator—"theories are necessary," partly as shorthand tools for a more meaningful organization of the already obtained facts but primarily as aids in a search for high-level facts to "explain" the lower-level ones. Autarchic isolationism and a view of "let each science show what it can do and then we will all get together" does not appeal to the writer on both pragmatic and ontological grounds. And blunders in the history of science are not confined to unempirical rationalists and intuitionists. Mach, the chief theoretician of "descriptionism," as is well known, opposed the atomic theory of matter for descriptive reasons.

Again, while the writer by no means subscribes to Pavlov's claim that the behavioral data of classical conditioning reveal the operation of a professed assemblage of specific brain mechanisms, he does maintain, as already stipulated, that CR data disclose general trends and directions of magnitudes and relative magnitudes of neural action, so that in a way Pavlov is right in classing conditioning as a method of studying the activities of the cortex. And at any rate, the writer is—and always has been—in full accord with the assertion of a growing number of his colleagues that whatever extra constructs the behavioral psychology of learning may need, they must be grounded in and modeled after neural action rather than merely fashioned in the form of abstract logical relations. True, the neural constructs have no predictive cash value (W. James, 180, p. 200; Feigl, 120) at the macrobehavioral level of conditioning proper, and are at present only promissory notes (Feigl *ibid.*) at the microbehavioral level of neural action. Yet—to continue the analogy—the debtor, the nervous system, is quite

real, of known location and unquestioned solvency, and has paid off on a number of occasions.

Hence, a formal neural systematization of the present account of classical CR acquisition (as a supplement to the behavioral systematization) would seem worth attempting. And the systematization as such is indeed an easy matter, either in terms of ratios of US- and CS-initiated neural events,  $US_N/CS_N$ , or better in terms of Neural Dominance, defined as a  $US_N/CS_N$  ratio that is greater than 1. Thus, we might simply say that the dominance-contiguity theory of the acquisition of classical conditioning involves, besides contiguity, four basic principles: Minimal Dominance, Optimal Dominance, Overdominance, and General Action Level. Minimal Dominance means that classical CR acquisition may be effected only when  $US_N/CS_N$  equals  $k$  where  $k$  is considerably greater than 1; Optimal Dominance asserts that CR-efficacy increases as  $k$  increases to  $o$ ; Overdominance states that CR-efficacy decreases as  $o$  moves to  $w$ ; while the principle of General Action Level merely declares that CR-efficacy also varies directly with the absolute values of  $US_N$  and  $CS_N$ .

Again, the very problem of the existence of optimal dominance might be interpreted either in terms of (a) mutual  $US_N-CS_N$  interference, i.e., optimal dominance ensues when interference is at its minimum, or (b) as an intrinsic characteristic of conditioning. Evidence for interference was adduced earlier while the hypothesis of a specific  $US_N/CS_N$  ratio of conditionability appears very plausible in the light of general knowledge of biological interaction, so that future research is likely to need be concerned only with the determination of the relative role of

the two factors. Then, a comparison of neural and the behavioral systematization would also seem to be in order. Clearly, the neural account is more economical, more easily integrable with other areas of knowledge, and in general rich in deductive and explanatory power and in heuristic value. Yet it is just as plain that, until data on direct observations of neural dominance are available, this account must remain merely directional and qualitative, and prediction must devolve upon behavioral parameters (the  $R$  of .85 is unmistakable evidence of the predictive powers of the latter). And, moreover, even after direct neural data had been obtained, a further or deeper level, a third level, of biochemical hypotheses would still have to be invoked.

Finally, two other aspects of the problem should be considered: (a) historical factors, and (b) cognition. The historical factors refer to the effects of hereditary dispositions, pre-experimental behavior, and trial-to-trial variations upon CR-efficacy. However, with respect to the first two factors, it must be remembered that the present account is concerned only with immediately preceding CR parameters or, paraphrasing a phenomenological statement, that it holds that the neural field during conditioning contains all the determinants of the conditioning. Hence, the two factors could be said to be of any specific predictive—and systematic—value only if they disclosed specific information about  $US_N$  and  $CS_N$  or, in practice, if their correlations with CR-efficacy were over and above (net) those obtained with the CS and the UR characteristics under discussion. But here another difficulty arises, namely, the concretization of the hereditary and pre-experimental variations into a reliably

measured entity with which CR-efficacy could be correlated. Pavlov's alleged personality types in dogs and some American correlations between conditioning and personality in human subjects are presumably attempts in this direction; but neither their methods nor results seemed too definitive—or at all definitive—for the purposes of the present analysis. Again, while there is no doubt that the relation of CSs to  $CS_N$ s and of USs to  $US_N$ s change in the course of conditioning, the predictive parameters of the present account are based only upon mean values of series of CSs, URs, and CRs. The growth of the CR curves is a separate problem and needs separate treatment.

On the other hand, the questions about cognition can be answered rather definitely. The writer's data for the parameters were, as already indicated, invariably taken from human subjects who did not know that they were being conditioned. Moreover, the fact that the functional properties of these data parallel so closely those of Pavlov, as well as the general arbitrariness of the classical CR methodology, suggests that Pavlov's dogs, too, are in the main "unknowing" subjects. When human subjects know that they are being conditioned, the usual CR parameters, particularly those of CS intensity and CS-US delays, largely lose their functional relatedness to CR efficacy. Such factors as "knowledge of stimulus relations" and "attitudes towards CR situations" emerge in their stead, but these factors are difficult to quantify and difficult to equate with changes in neural action. Hence the "laws" of classical conditioning may be said to be operationally significant only—or primarily only—as laws of "unknowing" learning—in human subjects perhaps

more often "preknowing" learning in the sense that the conditioning is effected before the subjects come to know about it—while "knowing" conditioning may well be so transformed as no longer to merit the designation of "conditioning."

#### RELATION TO OTHER THEORIES OF CLASSICAL CONDITIONING

Space permits only very brief statements about the relation of the theory—the parameters and the theory proper—to the theories of Pavlov, Guthrie, Hull, and Tolman.

1. *Pavlov* (341, 342). Viewed behaviorally, the present theory is no doubt most closely related to what may be found in the writings of Pavlov. Pavlov certainly stated that weak URs and very strong CSs are unconditionable and that strong CSs could become conditioned only by means of very strong URs, and he clearly recognized that both peripheral and central factors are important determinants of classical CR acquisition—setting up a number of experiments to study the former and expressing verbal approval of the latter. Then, he explicitly discussed the basic equivalence of intensive and temporal factors in conditioning—delaying USs as equivalent to weakening CSs—and his general neural view of conditioning as "strong brain centers attracting weak ones" could obviously be regarded as a pictorial way of expressing a theory of neural dominance. However, Pavlov's specific hypotheses are an entirely different matter, and they in *no* way chime in with those of the writer. His assumed series of pairs of opposable and into-each-other-transformable brain mechanisms—excitation-inhibition, irradiation-concentration, positive induction-negative induction, auto-induction-reciprocal induction,

tion, external inhibition-internal inhibition, static action-dynamic action—are really quite unparalleled in their patent flaunting of any principle of parsimony in scientific theorizing. And his later (after 1923) addition of a statistically unverified animal typology—inhibitable, excitable, labile, and inert types—and of special hypotheses of abnormal brain action—the paradoxical, ultraparadoxical, equalizing, and equivocal states—make the entire system even more unwieldy and more untestable. One might indeed say that Pavlov's system, unlike Hull's, suffers, not from an overloading, but from an underloading of logic. Yet it, unlike the latter, has a very wide empirical base with which Pavlov was fully familiar.

2. *Guthrie* (165). Besides the already mentioned argument that contiguity is a necessary but not a sufficient condition for CR acquisition, the writer differs with Guthrie in three other respects. (a) He by no means accepts the proposition that movement-produced stimuli play an exclusive or even a primary role in classical CR acquisition, and he in general feels that replacing "association of ideas" by "association of movements" bears only partial progress towards the systematization of learning. There is every reason to believe that the neural representations of exteroceptive CSs—and they no doubt are so represented—become as readily, if not more readily, linked to URs as do proprioceptive CSs, and the consistent variations of CR-efficacy with such CSs may well be construed as empirical evidence for the contention. (b) The writer is convinced that whole acts—which Guthrie seems to consign to a limbo of "nonpsychological"—are just as directly conditionable as are separate movements, in the same manner in

which melodies are conditionable aside from their separate tones, and meanings and judgments are conditionable irrespective of the words and sentences in which they are expressed. Empirical evidence for *per se* conditioning of wholes is available, although not abundant, while the theoretical argument would be that, despite their variance, wholes maintain specific neural representations and that *anything that is neurally represented may become a conditioning datum*. (c) Guthrie's insistence that all CRs gain full associative strength in one trial appears to the writer also to have a philosophical rather than an empirical flavor. There is, of course, no denying that the classical conditioning of *some* reactions in higher animals and in men may be effected under some favorable conditions in one trial and that increments with repetition often stem merely from the enlistment of more "conditioners"—or the involvement of more favorable conditions. But this is a far cry from any generalization that there are no efficacy differences in the very basic unit of learning between men and paramecia and among the vast hierarchy of responses of different levels in human beings and in higher animals. Certainly all the data on salivary conditioning run counter to it.

3. *Hull* (170, 171). The main systematic divergence of the present theory from that of Hull has already been stated, namely, the rejection of drive reduction as a primary and a universal condition of classical CR acquisition. A more specific discussion involving Hull's separate postulates cannot for lack of space be undertaken here. However, two short general comments about the system might need to be made: (a) its disregard of the structural differences of

learning situations in the search for functional similarities; viz., indiscriminately combining data from classical conditioning, operant conditioning, maze learning, and even verbal learning; (b) its generalized mathematical equations that are derivable only if results of some studies are selected and those of others neglected, and if no note is taken of the low correlations between the conditionability of different responses (even such a supposedly established generalization that CS-US delays of .2 to .5 second are optimal in CR acquisition is completely off the mark in Pavlovian food conditioning where the optima are 2.0 to 5.0 seconds [*supra*]).

4. *Tolman* (573, 574). Tolman has in recent years acknowledged the specific value of neural hypotheses and a specific existence of a "motor pattern" kind of learning which presumably he would admit proceeds according to principles of classical conditioning. On the other hand, the writer long ago gave up, as already noted, the view that classical conditioning "explains" all learning. Nonetheless, the writer's digressions from Tolman's system are very basic and considerable. For one thing, the writer would claim much wider regnancy for the "laws" of classical conditioning, namely, the entire field of "unknowing" and "preknowing" learning (*supra*) whether it be of the performance-act or of the movement-reflex variety, and he would say that these "laws" break down only when the learning is clearly of a "knowing" kind. For another, while the writer fully recognizes the validity of the distinction between performances-acts and movements-reflexes (he has always argued for conditionability differences between units of conditioning and reported empirical dif-

ferences between reflexive and attitudinal conditioning), he considers the latter as components of, and not (as does Tolman) pointer readings for, the former. What reflexes and movements and secretions are pointer readings for, are microevents in the nervous system rather than macro-activities of the wholistic organism. But this being the last paragraph of the article, it might perhaps be permissible to express the differences between the writer's and Tolman's thoughts in this area in somewhat informal terms. On one occasion Tolman stated that conditioning is a rather stupid form of learning. The label does not seem to the writer offensive and he would accept it if by stupid is meant noncognitive. However, the writer would maintain that stupid behavior is both more plentiful and more in need of change than smart behavior. Moreover, while conditioning may be stupid from the standpoint of the one who is being conditioned it may be very smart from the standpoint of the one doing the conditioning. And at any rate one may well argue that science's concern is actually only the analysis and control of stupid forces, and that a psychology that tries to deal only with an "understanding of understanding," leaving out the "understanding of ununderstanding," is of necessity limited, and is limiting its scope and significance.

#### SUMMARY

1. A critical and statistical analysis was made of data on classical CR acquisition in 300 experiments on salivary conditioning in dogs and in 10 studies of salivary conditioning in adult human subjects who did not

know that they were being conditioned.

2. The analysis of the animal experiments revealed four highly predictive behavioral parameters of the efficacy of CR acquisition, viz.: CS-intensity, UR-magnitude, CS-US and US-CS delays, and UR magnitude/CS-intensity ratios. And the analysis of the human experiments brought out three additional parameters: CS-attensity, UR-affectivity, and UR-affectivity/CS-attensity ratios (present techniques affording no evidence on the three extra parameters in animal subjects).

3. The analysis also suggested the operation of four neural principles in the effecting of the CR acquisition, viz.: Minimal Dominance, Optimal Dominance, Overdominance, and General Action Level. The neural principles were of no aid in specific predictions of CR-acquisition behavior, yet imparted to the empirical systematization economy and meaning, and rich deductive and heuristic implications.

4. The analysis disclosed further that contiguity is a necessary but not a sufficient condition for classical CR acquisition and that drive reduction is neither necessary nor sufficient for it.

5. The seven S-R parameters were only of very limited predictive value in human experiments when the subjects knew that they were being conditioned and thus involved the operation of factors of "perception of stimulus relations" and "attitudes towards the CR situation"—thereby suggesting that classical conditioning is primarily meaningful (verifiable) only when it is conceived as an "un-knowing" level of learning.

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## INTRODUCTORY NOTE

Of the 668 items in the bibliography, 618 are reports of Russian experiments of salivary conditioning of dogs, and of these, 134 were published before 1927—the date of publication of Pavlov's text, and 293 before 1936—the year of Pavlov's death. The 334, or 54.1% of total, post-Pavlovian experiments in the special area under discussion, must not, however, be taken as a representative index of the relative amount of Russian experimentation in the post-Pavlovian and Pavlovian periods in conditioning in general, inasmuch as the post-Pavlovian period has manifested a considerable shift of CR interest to: (a) non-salivary autonomic conditioning, (b) interoceptive conditioning, (c) conditioning of human Ss, and (d) conditioning of animals other than dogs. When account is taken of all Russian classical CR studies, the post-Pavlovian/Pavlovian ratio is, in the writer's estimation, likely to be close to 2:1, and, it might be added, the production curve continues to be positively accelerated.

The bibliography does not include "abstracts," and it omits also 21 "quoted" experiments, the original reports of which were not available for examination, besides an estimated number of about 60 studies which the writer thinks have altogether escaped his notice. Moreover, the bibliography is confined to experiments with dogs in a simple "Pavlovian frame," thus leaving out studies of salivary conditioning of free-moving dogs and of dogs in a "bilateral Anokhin frame," that is, a frame in which the animals are fed sometimes from the right and sometimes from the left side, usually with different CSs in each case. Again, experiments published in more than one place were not repeated by title but entered as single items of multiple references. And experiments the data of which were treated statistically are designated by the following four letters in front of the numbers of their references: *i*—treatment of data for CS-intensity, *m*—treatment for UR-magnitude, *im*—treatment for both CS-intensity and UR-magnitude, and *d*—treatment for CS-US and US-CS delays; while the letter *a* is placed in front of articles containing descriptions and diagrams of apparatus. Finally, the bibliography contains also, in parentheses, references to each Russian article abstracted in the *Psychological Abstracts*; but it should be added that only 134 of the 478 post-1927 Russian articles of the bibliography, or only 28%, were so abstracted by the journal.

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## THE CALIFORNIA F SCALE IN PSYCHOLOGICAL RESEARCH: 1950-1955

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With the publication of *The Authoritarian Personality* (2) in 1950, the commonality of two streams of research interest was established. By applying the tools of "depth psychology" to the study of ideology, this work opened the way for a substantive integration of personality dynamics with social behavior.

While it is too early now to foretell the eventual place accorded this work, it may certainly be said to have left its mark on the contemporary scene. Probably no other single development in recent years has stimulated so much thought and investigation. Since 1950, a veritable cornucopia of research in the authoritarianism area has evidenced itself. Even as the outpouring continues, however, we may reasonably hope to take stock of the accomplishments of this period.

More than any other, the F (Fascism) scale developed by the California group has been widely accepted and widely used as a research variable. It is the purpose of this paper to present an overview of studies using the F scale which have been reported in the literature. Since the scale (Form 40-45) has been modified in one way or another by many of its users, it has occasionally been necessary here to make somewhat arbitrary decisions concerning the inclusion or exclusion of a study. Generally, only those studies which have not changed the scale too severely have been included; thus, data drawn from the A-E scale are not covered, as is true of other extremely

short modifications of the original. However, some studies using 12 or more of the most discriminating items of the original have been included. The guiding principle for decisions of inclusion or exclusion has been the intent of gathering together that information which will be most valuable to future users of the F scale and which will contribute most to an understanding of its meaning.

To facilitate presentation and comprehension, studies have been grouped into five major categories, with appropriate subgroupings. Other segmentation schemes may have served better; this procedure offers advantages, but assuredly has limitations as well.

### IDEOLOGY

It is convenient to distinguish studies with an ideological, or attitudinal, flavor from those striking more deeply at personality characteristics. In this section our focus of attention will be on the former grouping.

#### *Prejudice*

Despite the fact that the F scale was originally developed as a measure of prejudice, few studies have dealt with its relationship with prejudice per se. Flowerman *et al.* (23) made a study of the validity of the F scale as a measure of ethnic prejudice for groups geographically removed from those on which the original validity studies were conducted. They found correlations ranging from .30 to .60 between the F scale and prejudice.

The subjects were Protestant and Catholic men and women in Minneapolis, St. Paul, and Baltimore. It is unfortunate, at least for comparative purposes, that an interview method was used for determining the subjects' F-scale score, thus making extrapolation to the questionnaire method difficult, if not impossible. The results, however, did tend to support the use of the F scale as a measure of prejudice.

Goldstein *et al.* (26), using the Allport-Vernon Scale of values, a social distance scale, and a background information questionnaire, investigated the relationship between prejudice and other ideological aspects of the authoritarian syndrome using groups of white, nonsouthern, native-born, Christian students from Princeton and UCLA. Goldstein confirmed that a general tolerance-prejudice factor exists and that an authoritarian syndrome closely associated with prejudice is quite real. It is important to note that this author felt that the approach of Adorno *et al.* ". . . is tenable though quite unsubstantiated. However, [their] great desire to theoretically unify authoritarianism seems mistaken. . . . The authoritarian syndrome is probably a conglomerate which cannot be entirely explained on the basis of an elaborately unified theory. . . . If this desire [to unify] were kept within reason, abnormality would probably receive less emphasis and the theoretical contribution of the authors would be of greater value" (26, p. 35).

Somewhat by way of contrast, Campbell and McCandless (10) report evidence for the existence of a general factor which tends to unify "most, if not all" attitudes toward those of other ethnic groups. They provide evidence that the F scale correlates substantially with a variety

of prejudice measures, including ethnocentrism and xenophobia.

Steckler (60) conducted a study of the authoritarian ideology in Negro college students. Scales were developed to measure anti-Negro attitudes, antiwhite attitudes, and so-called ideological militancy-pacifism. He found a moderate covariance between anti-Negro, antiwhite, and ideological militancy-pacifism on the one hand and F on the other. Steckler suggests that authoritarian components must therefore be involved, to some degree, in this measure. Since the F scale is usually used with white, Christian, nonsouthern Americans, it is difficult to evaluate these data. We have, at present, no evidence to indicate clearly the meaning of F-scale scores for Negroes. Proper evaluation must await further work using F or similar scales with Negro populations.

#### *Political Attitudes*

At the time of the 1952 national political conventions, Milton (47) hypothesized that ". . . individuals who preferred MacArthur for President of the United States would obtain higher scores on the F scale . . . than those who preferred other presidential aspirants" (47, p. 597). To test this contention, he administered the F scale and a questionnaire concerning presidential choice to 390 students at the University of Tennessee. In line with his hypothesis he found that those students who favored MacArthur had significantly higher F-scale scores than those favoring other candidates. In addition, he found that a significant number of the students favoring either Taft or MacArthur were in the upper 25 per cent of F-scale scorers. Milton warns that these data must be interpreted with caution, but suggests that there

seems to be evidence "... for a speculation that has been offered by many individuals, i.e., voting behavior is influenced, partially, by certain personality attributes of both the voter and the candidate" (47, p. 598).

Gump (28) noticed that newspaper comments in opposition to Truman's dismissal of MacArthur seemed to follow the antidemocratic trends as defined in *The Authoritarian Personality*, and tested the proposition that high F-scale scorers would favor MacArthur (an authoritarian symbol) whereas, the low scorers would tend to favor Truman's action. The statement "MacArthur should have been dismissed" was added to the F scale which was administered to a sample of 405 students at the University of Colorado. The results showed that those favoring MacArthur had significantly ( $p < .001$ ) higher F-scale scores than did those who favored Truman's action. Gump suggests that "Insofar as the F scale measures anti-democratic trends, the hypothesis that there is a relatively high frequency of anti-democratic trends among these college students who opposed the dismissal of General MacArthur is amply supported by the findings" (28, p. 134). This study, like the preceding one, is based on the premise that students with relatively high authoritarian tendencies will be attracted to authoritarian symbols. Since the results were in both cases significant and in the direction predicted, it appears that the F scale may have some value in predicting political trends; however, it is likely that such clear-cut results can be obtained only when individuals who have been clearly presented as authoritarian figures are involved.

#### *Family Ideology*

Huffman (31) has developed a

scale for the measurement of "Traditional Family Ideologies" (TFI) which correlates .73 with a shortened version of the F scale. This author suggests that to the degree that the F scale and the TFI scale are valid, the correlation between them would indicate a fair correspondence of what is being measured by the two scales. Since the hypothesis was that the autocratic trends in family ideology could be shown by an attitude-opinion scale, and in turn related to the authoritarian personality variables, these results would seem to indicate some success in demonstrating her hypothesis.

Kates and Diab (37) hypothesized a correlation between attitudes on parent-child relationships and authoritarianism. They administered the F scale, The University of Southern California Parent Attitude Survey, and a number of other scales including the California Ethnocentrism and Politico-Economic Conservatism scales and a scale of ambiguity intolerance, to 172 college students (61 males and 111 females). Of interest here is their finding of a correlation of .21 ( $p < .01$ ) between the F-scale scores and possessive attitudes, for males and females combined. For the sexes separately, they found that the female students' F-scale scores correlated .34 and .28, ( $p < .01$ ) respectively with dominant and possessive attitudes, whereas for the males the only significant correlation with the F scale was a .29 ( $p < .05$ ) correlation with ignoring attitudes. The authors tentatively suggest, from these and other relationships found, that "strong authoritarian beliefs may be maladjustive in a democratic society since such beliefs are associated with attitudes similar to those held by parents of problem children" (37, p. 16).

*Teacher Attitudes*

Juul (36) has considered F-scale scores in relation to teacher attitudes toward child behavior. He used a shortened version of an inventory called "How I Teach" to measure teaching attitudes of college seniors in a teacher training curriculum. It was his hypothesis that ". . . many of the concepts and attitudes expressed by teachers and textbooks in courses in child psychology and mental hygiene constitute a serious threat to the equilibrium of the inner world of the authoritarian individual. In organizing his experiences, he is therefore forced to exclude, distort, repress or otherwise fit every situation, event, and person into his framework and need structure" (36, p. 17). As a consequence of this hypothesis it would naturally follow that equalitarian students would tend to get significantly higher scores than authoritarian students on a test of knowledge of child psychology, teaching attitudes, and opinion-teaching practices. As was predicted, he found a negative correlation between "How I Teach" and the F scale. In addition he found that F-scale scores tended to decrease and "How I Teach" scores to increase as a function of the number of courses taken in psychology; however, this relationship was not significant. As a result of his findings, Juul suggests that "In the interest of children, students who display extreme authoritarianism should be encouraged to enter some field of endeavor that does not entail constant contact with children" (36, p. 189). Also he suggests that teachers should be helped to understand the causes of an authoritarian personality and be made aware that many practices within the classroom tend to aggravate these tendencies toward authoritarianism.

*Attitude Change*

Christie (11), using 182 white inductees in an army basic training center, attempted to find the relationship between involuntary membership in a military society and individual acceptance of authoritarian statements. A modified form of the F scale was given in conjunction with interviews prior to and after completion of six weeks of infantry training. Only a minor increase in acceptance of F-scale items was found upon the second administration of the F scale. However, when the recruits were divided into subgroups on the basis of sociometric data, it was found that those recruits who were more accepted than rejected by both peers and training personnel did shift significantly ( $p < .05$ ) toward higher F-scale scores. The increase in authoritarianism by those recruits who fit better into military life was taken to indicate the importance of situational as well as personality factors in the acceptance of the authoritarian ideology. Although it is no doubt true that situational factors are important, one cannot draw definite conclusions until the problem of the relationship of authoritarianism and military adjustment is better understood.

Levinson and Schermerhorn (39) used the F scale together with several other variables in a study of the emotional-attitudinal effects on its members of a workshop in intergroup relations. In the test given at the beginning of the workshop they found authoritarianism to be somewhat greater (a) among Negroes than among whites, (b) among teachers as compared to other occupations, (c) among Catholics than among "conventional" Protestants, and (d) among "conventional" Protestants than among humanistic Protestants.

(Unitarians and Congregationalists) and Jews. Of particular interest is the fact that they found a significant decrease ( $p < .05$ ) in F-scale scores on the second testing which was done at the end of the workshop. Nevertheless, the over-all trends remained the same, as was demonstrated by a correlation of .75 between the initial and final testings. For those who are interested in changing prejudicial attitudes this study should offer some hope; however, only the fact that these attitudes can be changed in a relatively short period of time has been demonstrated. Data are still needed to provide information concerning the best means of changing these attitudes and perhaps of greater importance, we need data concerning the relative stability of these attitudinal shifts.

A recent study of Wagman (62) seems to shed some light on the problem of method of changing the prejudiced attitudes of relatively authoritarian and nonauthoritarian individuals. It was found that for relatively high F-scale scorers, attitudes of racial prejudice were increased or decreased by use of a technique of authoritarian suggestion. Although a nonauthoritarian information method was effective with low F-scale scorers, it tended to "boomerang" for the high scorers. These findings are provocative and may lead to a greater understanding of attitude change; however, they are not unequivocal. Although in most cases Wagman's results were in the direction predicted, in a number of cases they failed to reach the .05 level of significance.

#### PERSONALITY CORRELATES *Social Perception*

Scodel and Mussen (59) report a study intended to give additional in-

formation on the characteristics of authoritarian perception. Specifically, they wished to test the hypothesis that "... authoritarians, because of their lack of insight into others and their need to consider themselves members of the ingroup, would perceive nonauthoritarian peers to have attitudes and personality characteristics similar to their own" (59, p. 184). They studied 27 pairs of subjects, each consisting of an authoritarian and a nonauthoritarian, as defined by F scores. These pairs were told to discuss such neutral topics as radio, television, and the movies. At the conclusion of these sessions, subjects were given a second administration of the F scale with instructions to respond as they thought their discussion partners would. In line with the hypothesis, the high authoritarians did not perceive their partners as having F-scale scores differing from their own; whereas those with low F-scale scores estimated their partners' scores to be significantly higher than their own, but lower than they actually were. The low F-scale scorers were significantly more accurate in their estimate of their partners' responses on both the F scale (and the MMPI) than were the high F-scale scorers. The results, therefore, are in accord with the hypothesis.

In a study of the stability of the self concept, Brownfain (9) thought it desirable to distinguish between rigid and stable individuals. Accordingly, he used the F scale to identify rigid individuals so as to eliminate them from his sample; he found the correlation between F-scale scores and his index of stability for this sample of 62 to be  $-.25$  ( $p < .05$ ). Thus, in accordance with his expectations, the lower (more stable) a per-

son's score on this index, the higher his F-scale score.

By way of contrast to Brownfain's finding, however, Cowen (19) found that low negative self-concept scores went with high F scores. These results were equivocal. In one group ( $N=34$ ) he failed to find a significant difference, whereas in another ( $N=47$ ) he found differences in authoritarianism between high and low negative self-concept scorers to be significant at the .01 level. Cowen suggests the generalization that high scorers, i.e., those with a marked negative self concept, are less predisposed toward authoritarian attitudes.

#### *Rigidity*

Brown (6, 7, 8) reports a study of the relationship between rigidity and authoritarianism. Since in exploratory studies he failed to verify Rokeach's findings of the relationship between problem-solving rigidity and authoritarianism, he compared their respective procedures and found indications that it might be necessary to arouse some anxiety over achievement before authoritarian subjects would perform rigidly on the Einstellung arithmetic problems. It was hypothesized that this anxiety would be aroused when the testing orientation was ego-involving. Using the ego-involving nature of testing atmosphere as the independent variable, he administered the F scale, the Einstellung arithmetic problems (as a measure of rigidity), and McClelland's projective measure of need for achievement (to provide an index of anxiety over achievement), to a group of college students.

As expected, there was a greater correlation between the score on the Einstellung problems and the F-scale score in the group that was ego-involved than in the group that was

relaxed. It is interesting to note that in the group that received an ego-involving orientation, both authoritarianism and rigidity were associated with anxiety over achievement, while in the relaxed group, moderate anxiety scores were associated with high F-scale scores, but not with greater rigidity in problem solving. Brown suggests that these results indicate that the rigidity which is associated with authoritarianism is a kind of defense mechanism which is perceived as warding off personal failures.

Jones (35) has studied the relationship between Necker Cube fluctuation and F-scale performance. Drawings of the Necker Cube were presented to two groups of Naval Aviation Cadets. One group ( $N=251$ ) was given a set toward reversal, while a second group ( $N=122$ ) was given a set against reversal. Kendall's rank-correlation procedure was used to compare F-scale scores with the number of reversals reported by each cadet. In the first group, a correlation of  $-.144$  (which corresponds to a normal estimate of  $r=-.22$ ) was found when the cube was drawn on the board. Similar results were found when each cadet had a drawing of the Necker Cube on a sheet before him. In both cases the null hypothesis was rejected at the .01 level or better; however, in the second group the rank correlation between the two variables was only  $-.017$  (which corresponds to a normal estimate of  $r=-.03$ ). By way of interpretation, Jones suggests that the authoritarians are intolerant of fluctuation, while the nonauthoritarians are not so disposed. He concludes that "Since the F scale is a measure of authoritarian attitudes, an empirical relationship between the F scale and a perceptual variable speaks strongly

for a personal base to authoritarian attitudes. . . . Only by supposing a personal substructure underlying the adoption of authoritarian attitudes can we account for the observed empirical relationship" (35, p. 126).

Because there have been conflicting results concerning whether the authoritarian is more rigid and more intolerant of ambiguity than is the nonauthoritarian, Davids (21) has attempted to clarify the issue. He administered the F scale, the Taylor Manifest Anxiety Scale, ambiguous visual stimuli (the inkblot concepts from McReynold's Concept Evaluation Technique) and ambiguous auditory stimuli (selections from the Azzageddi Test) to a group of twenty male college students selected on the basis of the heterogeneity of their backgrounds. In addition, information was gathered concerning each student's academic achievement and his "ego-structure" (the latter was based on the judgment of a clinical psychologist).

It was found that the F-scale scores correlated positively with manifest anxiety and negatively with academic achievement and ego-structure. All of these correlations were statistically significant. However, contrary to theory and the experimenter's expectations, no significant relationship was found between the F-scale scores and tolerance of either ambiguous visual stimuli or ambiguous auditory stimuli. Also, a direct measure of ambiguity tolerance based on the subject's ratings of their own reactions to the auditory task showed no significant relation with F-scale scores. These data would seem to add more weight to the point of view that intolerance of ambiguity is not part of the authoritarian syndrome. Nevertheless, since various investigators have used different instruments and

have employed greatly differing samples, their data cannot be combined to give a clear-cut answer to the problem. At present the available evidence is spotty and equivocal.

#### *Neuroticism*

Jones (34), in a study of the relationship between manifest anxiety and authoritarianism, administered the F scale and the Taylor Manifest Anxiety Scale to two samples ( $Ns$  of 166 and 245, respectively) of Naval Aviation Cadets. For the two samples he found significant rank correlations of .134 ( $p < .015$ ) and .143 ( $p < .003$ ), respectively. Jones notes that while positive correlations were found, nevertheless, they are rather low. This can be explained, he suggests, on the basis of three considerations. First, the F scale measures authoritarian ideology and thus high scores may not always reflect authoritarian personalities, but instead authoritarian inclination. Second, among the low F-scale scorers there may be "genuine authoritarians of some other sort than that measured by the F scale" (34, p. 3). Finally, anxiety may accompany a number of personality disorders, many of which are not in themselves authoritarian. Jones concludes with the view that, if the presence of anxiety may be taken as an index of underlying neurosis, authoritarianism may be regarded as a neurotic form (34, p. 4).

Taking a point of view which is diametrically opposite to Jones' conclusion, Masling (43) suggests that the concept of the authoritarian personality can have utility only when it is devoid of what he considers to be value judgments. Despite the fact that some observers have viewed the authoritarian as neurotic, Masling refers to four studies which have failed to demonstrate that the au-

thoritarian is significantly different from the equalitarian with respect to neuroticism. Of particular interest here is a study which Masling made with Courtney and Greer (18) in which they failed to find a significant correlation between the F scale and the Rotter Incomplete Sentences Test. When high and low F-scale scorers were compared in terms of their incomplete sentences, there was no significant difference between means. Masling concludes that there is a tendency to use the term "authoritarian" as a mild form of profanity to describe undesirable persons. He suggests that the concept be re-examined in the light of these data and be purified of value judgments before further work is done using it as an anchorage for research (43, p. 318).

Stotsky (61) has presented evidence that, although existing data seem to show that there is not a relationship between neuroticism and the F-scale scores, students in psychology classes are learning a neurotic stereotype of the authoritarian. Students ( $N=137$ ) enrolled in classes of adjustment were presented with the F scale three times. The first time they were told to respond according to their personal opinions; the second time as a "neurotic individual" would; and the third time as "the normal person" would. Critical ratios showed significant ( $p < .001$ ) differences between the means in each of the three possible comparisons. The students rated themselves lower than "normals" and the "normals" lower than the "neurotics." The author suggests that this indicates the presence of a stereotype regarding the authoritarian which follows on the tendency, described by Masling (43), to view "authoritarian" as somehow "bad."

### *Impulsivity*

Sampson (58) undertook an investigation to determine the differences in chance-taking behavior between authoritarians and nonauthoritarians. Using as subjects 93 boys and girls in a high-school college-preparatory program, he conducted two gambling experiments, after which he administered the F scale, a questionnaire concerning behavior in the gambling experiments, and the MMPI.

Contrary to his expectations, no differences were found between authoritarian and nonauthoritarian subjects in their mean chance-taking behavior. Also, he found "no consistent evidences . . . for a relationship between mean chance-taking behavior and impulsivity . . ." (58, p. 96). A significant tendency was found, however, for inconsistency in chance-taking behavior to be associated with low authoritarianism. There was evidence, too, that persons who are inconsistent in their chance-taking behavior obtain higher impulsivity scores. Sampson suggests that "the inconsistent behavior of authoritarian persons may have been due to the pressure of such conflicting factors as their estimation of the most logical behavior according to probabilities, their desire to hold to what they could be sure of, and perhaps to 'hunches' that they might be lucky. The more impulsive, loosely organized behavior of authoritarian persons is interpreted as a resultant of their confusion and conflict in a situation of manifest uncertainty" (58, p. 97). It should be emphasized that these results do not necessarily support the contention that authoritarians are intolerant of ambiguity.

### *Miscellaneous*

Radke-Yarrow and Lande (50)

have investigated the relationship of personality and reactions to minority group-belonging. Sixty Jewish men and women of college age were studied. Each subject was given a battery of tests covering personality and group-belonging variables. The F scale was found to be related to reactions of group-belonging: individuals with high F-scale scores were much more anti-Semitic ( $p < .001$ ) and showed more avoidance of group-identification ( $p < .03$ ), than were individuals with low F-scale scores. A comparison of high and low F-scale scorers also revealed that those persons who scored high were more aggressive and less constructive in their reactions to problems arising out of minority-majority group conflicts. Finally, it was found that in this sample the high F-scale scorers also scored high on a scale of social distance.

It is noteworthy that Adelson (1) has developed a scale of Jewish Authoritarianism which correlates .67 with the F scale. This scale is said to be internally consistent and have a corrected odd-even reliability of .79. Adelson has found this scale useful in differentiating certain subgroups within his Jewish college student sample.

Rosen (56) has compared volunteers and nonvolunteers for psychological experimentation. He found that volunteers evidenced significantly lower scores on the F scale than did nonvolunteers. Furthermore, volunteers were found to take significantly longer to complete his questionnaire. It is conceivable, however, that the differences were more a function of interest than of authoritarianism, that is, the volunteers may have been trying to obtain "better" scores.

Meer (45, 46) has presented evi-

dence concerning the affective content of the dreams of high F-scale scorers as opposed to low F-scale scorers. For his sample of undergraduates, he found that high scorers have more aggression toward outgroup characters than toward ingroup characters ( $p < .01$ ); in the dreams of the low scorers, there was no significant difference in aggression between ingroup and outgroup characters. In addition, he found that high scorers have more friendly acts with ingroup characters than with outgroup characters in their dreams ( $p < .05$ ); no such differences were found for low scorers. Meer indicates that the significant differences obtained support in part the contention that authoritarians cannot tolerate emotional ambivalence.

#### INTERPERSONAL BEHAVIOR

This section is concerned with behavioral correlates of authoritarianism, particularly as they are manifested in personal interactions.

##### *Leadership*

The relationship between authoritarianism and leadership has been the focus of attention in a number of studies. Military and industrial needs for leaders are everpresent, but the Korean war heightened these needs at about the same time that *The Authoritarian Personality* was published. On the surface, at least, there appeared to be a relationship between authoritarian ideology and leadership, especially in the military institution which traditionally has been seen as authoritarian.

Rohde (53) tested the hypothesis that authoritarians are more likely than equalitarians to be accepted as followers by their superiors. The F scale was administered to each of 176 aircrew members. In addition, each

man was rated by his airplane commander according to three standards: (a) willingness to take the man into combat, (b) the man's desirability as a friend, and (c) the commander's confidence in the man. Contrary to expectation, negative correlations (-.326, -.457, and -.112 respectively) were found between these standards and F-scale scores. These results appear somewhat puzzling in light of theoretical considerations regarding authoritarianism. One might have expected that leaders would tend to select those individuals who would readily submit to their authority (authoritarian submission).

Further unexpected results were found by Hollander (29, 30) in a study with Naval Aviation Cadets. He hypothesized that (a) there would be a significant positive relationship between scores achieved on the F scale and incidence of choice by one's peers for a military leadership position, and (b) there would be a significant difference, with respect to F-scale scores, between the leadership nominees of those "high" on authoritarianism and those "low" on authoritarianism (30, p. 365). A total of 268 cadets from nine consecutively formed sections at the Naval School of Pre-Flight were asked to fill out a leadership nomination form for their section in which they were instructed to pick the three best qualified and the three least qualified to serve in the hypothetical position of "Student Commander." The peer-nomination scores evolved were related to F-scale scores. Correlational analysis revealed a negative relationship between authoritarianism and peer nominations ( $r = -.23$ ;  $p < .01$ ) which remained significant even when intelligence was held constant. Again contrary to expectation, no significant differences were found between

those subjects nominated as leaders by others who were respectively "high" and "low" on authoritarianism. In essence, Hollander suggests that a high F-scale score may indicate a person's lack of social intelligence or social perception, thus making him less able to deal effectively with the needs of others and consequently less acceptable to his peers as a leader. Another consideration put forth is that there may be real differences between acceptance of the authoritarian ideology and authoritarian behavior as it is traditionally conceived.

Bass *et al.* (5) have hypothesized that personality variables which tend to differentiate between high and low authoritarians would also differentiate between those who score respectively high and low in leaderless group discussions (LGD). The F scale was administered to a group of ROTC students for whom LGD ratings were obtained the previous year. A curvilinear relationship was found between these two variables (eta was significantly greater than Pearsonian  $r$  at the .01 level). However, the authors note that the obtained Pearsonian  $r$  of  $-.16$  ( $p < .05$ ) indicates "... that there existed a slight, general tendency for those who score higher on the LGD to be more resourceful, flexible, and self-reliant, and less stereotyped, submissive, power oriented, and rigid individuals than those who scored lower on the LGD" (5, p. 127). They report that an inspection of the scatterplot of these data revealed that the extreme authoritarians did very poorly as a group on the LGD. Those who achieved maximum success on the LGD were the subjects that tended to be permissive but not overly so. Finally, those who were extremely tolerant tended to score low on LGD,

but not as low, however, as the more authoritarian subjects. While these interpretations are worth noting, one may well feel that they are founded more in theory than in the available empirical evidence.

In still another relevant work, Jones (32, 33) has studied authoritarianism as it relates to the perception and evaluation of potential leadership. Ten groups were set up, composed equally of high and low F-scale scorers. Each group was presented with information about a recruit platoon leader by means of a mock interview recorded and played to the subjects. The information given was varied along two dimensions: personal power (forceful or passive) and leadership attitude (autocratic or democratic). Subsequently, they were asked to rate the potential leader on a 30-trait rating scale. Contrary to his prediction, it was found that low authoritarians saw the forceful stimulus person as more powerful and the passive stimulus person as less powerful than did the high authoritarians. In an interpretation similar to Hollander's (30), Jones suggests that this difference may be a function of the relative sensitivity of the lows to personal or internal power cues as compared to the highs who tend to differentiate on the basis of institutional status. The high authoritarians also tend to be more accepting and less critical in their ratings and descriptive comments than do those who obtain low F-scale scores. Moreover, it was found that those respectively high and low in authoritarianism differ in their organization of an impression as measured by differences in the intercorrelations of traits, but this organization was found to be highly dependent on the nature of the stimulus information presented. The lows

tended to evaluate leadership on the basis of many different personal and performance qualities, whereas the highs tended to make a clear distinction between the merits of the stimulus person as a leader and his worth as a person. Contrary to Jones's prediction the lows demonstrated a more pervasive "halo effect" than did the highs; however, the lows were seen to be at the same time more sensitive to stimulus cues and generally more critical of the stimulus person.

Medalia (44) has produced the most recent investigation of authoritarianism and leadership. With a sample of airmen ( $N=298$ ) in Air Force radar squadrons, he tested several hypotheses having to do with the interplay of F scores with leader acceptance and group cohesiveness. He used re-enlistment intent as an index of the latter variable. One of his major predictions was that the correlation between leader acceptance and re-enlistment intent would increase with higher F scores. It was found, however, while there was a considerable increase in correlation between these two variables for the middle level of F-scale scores, as compared to the low level, that the trend did not continue; the correlation was very low for the high F-scale scorers and, in fact, slightly lower than was obtained in the case of the low F-scale scorers (44, p. 211). But beyond this, Medalia has provided data indicating a positive relationship between leader acceptance and the level of unit member authoritarianism. Furthermore, a negative attitude toward re-enlistment was found more often among low authoritarians than among high authoritarians. The central test of the "Frommian compromise" (44, p. 212) raises more questions than it answers, however.

To deal with some of the problems

associated with the relationship between authoritarianism and acceptance of military ideology, French and Ernest (25) studied 186 airmen undergoing basic training. The F scale, some items similar to the F scale, and a "military ideology" scale were administered once at the beginning of training and again at the end of six weeks. Half of the subjects were tested under allegedly anonymous conditions, while the other half signed their papers.

In accordance with their hypothesis they found that F-scale scores were related to acceptance of the military ideology. It was also found that the relationship was somewhat stronger when the items dealing with projectivity, sexual goings on, superstition, and so on, were omitted, that is, when only the items dealing with attitudes toward authority, conventionalism, and hard-headedness were used. This may be a clue to reasons for some of the unexpected results found by others. Indeed, it may suggest that the F scale attempts to measure too much, at least insofar as military adjustment and leadership are concerned. It is also of interest that the "F scale responses were found to be unchanged by military training, but subject to increase under the pressure of the combination signing the papers and the first week of training" (25, p. 191).

On theoretical grounds, the use of the F scale in studies of leadership appears entirely appropriate. In point of fact, though, the meaningfulness of the empirical data obtained is not always evident. One may even say that, since both variables are so complex and diffuse, we are correlating one unknown with another.

### *Group Behavior*

In a study by McCurdy and Eber (41), groups with either an authoritarian or democratic orientation were established. In addition, one each of the two kinds of groups was made up of high F-scale scorers while the other was made up of low F-scale scorers. All groups were given the problem of learning the correct pattern of switch order necessary to keep a bulb lit in the center of the table at which they were seated. Contrary to expectation, no significant differences were found between groups, except in the case where errors per unit of time were computed; there the *F* ratio for the significance of differences was significant at the end of one and two minutes, that is, when the groups were still actively at work trying to solve the problem. The authors suggest that it is possible that the non-significant results may have been a function of the disturbance caused by placing authoritarians in a democratic setup. This contention still requires experimental demonstration.

Wells (64) has investigated a number of possible behavioral correlates of the authoritarian personality. He collected detailed records of the behavior of individuals, respectively high and low scorers on the F scale, from observations of small discussion classes. Of his twelve hypotheses, only one was supported by the data; this was the prediction that high F-scale scorers would check more of the complimentary responses on a "Comments on the Course" questionnaire. The over-all conclusion from this study was that perceptual and cognitive differences cannot be clearly translated into behavioral terms. This conclusion adds to the position that there is an incongruity between

authoritarian attitudes and authoritarian behavior, as has been suggested elsewhere (30).

#### CULTURAL AND SUBCULTURAL VARIATIONS

##### *Variation Within the United States*

Christie and Garcia (12) noted that inasmuch as a wide range of attitudes and ideologies regarding minority groups are overtly expressed in California, subjects from this population would have greater freedom to select ideologies which are compatible with their need for outlets for aggressive impulses. As a consequence, it seemed reasonable to determine whether similar patterns would be found in a subculture marked by relatively more homogeneous ideologies. They compared, therefore, the F-scale responses of a group of students at the University of California—presumably comparable to the original sample of Adorno *et al.* (2)—with a college population in an unnamed southwestern city. The latter population, although characterized by similar demographic variables, was said to have had a more limited ideological exposure. They found that the southwestern city sample had significantly higher acceptance of items on the F scale. Furthermore, an item analysis of the F scale indicated that 18 of the thirty items were accepted to a significantly greater extent ( $p < .05$ ) by the southwestern city students (12, p. 469). A cluster analysis of the responses on the F scale yielded three clusters which were similar in both groups, but the remaining four California clusters were not duplicated by the five remaining southwestern city clusters. It was found, too, that the same item often fell in different clus-

ters in the same samples; therefore, it was suggested that interpretation of the meaning of an item must vary between the two subcultures (12, p. 465).

Bass (5) in the study of the LGD mentioned above, under leadership, used as subjects students from the deep South. He found that in general there was not much difference in mean scores between this and the original California group. It was noted, however, that the range in scores for his subjects was quite restricted. This may have served to attenuate the correlation.

Davidson and Kruglov (22) have investigated the possibility that certain background variables have an effect on F-scale scores. They administered the F scale and a background questionnaire to a sample of 492 male students enrolled at the City College of New York. For purposes of comparison, they selected groups of high and low scorers on the F scale ( $1 SD$  above and below the mean, respectively). They found no significant differences between high and low scorers with respect to parents' birthplace or occupation, with the exception that significantly more ( $p < .05$ ) students with one foreign-born parent were in the low scoring group. They did find significant differences between high and low scorers with respect to interest in college: significantly more ( $p < .01$ ) of the students majoring in engineering were among the high scorers than were among the low scorers. Also a significantly larger proportion ( $p < .01$ ) of the students majoring in liberal arts were low F-scale scorers. Davidson and Kruglov suggest that some of these background variables may account for similarities and differ-

ences among persons exposed to different subcultures.

#### Foreign Samples

Prothro and Melikian (49) set out to determine whether ". . . residence in an authoritarian culture [leads] to greater acceptance of the 'authoritarian' items of the California scale" (49, p. 354). They suggest that this problem has implications regarding the validity of the scale. In addition, their study also attempts to answer the question of whether an authoritarian culture produces other traits found in the antidemocratic personality, such as Politico-Economic Conservatism. Using 22 F-scale items, plus items from other scales, the authors tested 130 subjects (70 Christians and 60 Moslems) at The American University, Beirut, Lebanon. A mean F score of 5.03 was found which was significantly higher than that found in studies with American students. The Moslems had a mean score of 5.15, the Christians 4.86; a *t* between these was significant ( $p < .02$ ).

Since a somewhat abbreviated scale was used, it was not legitimate to make direct statistical comparisons between this sample and American samples. However, the authors suggest that the difference would probably be highly significant. It is interesting to note that in this sample, unlike American samples, the correlation between Politico-Economic Conservatism and the F scale was quite low ( $r = .02$ ). Despite the fact that the F scale was not used in its conventional form, the data clearly indicate a marked cultural variation in F-scale scores and at the same time lend some weight to the validity of the F scale as a measure of authoritarianism.

Another study, this one by Cohn and Carsch (17), used the F scale with a sample of 140 workers in a

German cosmetics factory. For this group, a mean score of 5.26 was obtained. "This score is higher than the mean score for any group thus far reported in the literature. . . . If one makes the assumption that these workers are 'fascistic' personalities, then the data appear to support the validity of the F scale" (17, p. 471). The authors also found a negative correlation between educational level and F-scale scores; this is comparable to the trend found with American subjects by Cohn (15), among others.

#### STUDIES OF THE INSTRUMENT

Since it is an implicit assumption in studies using the F scale that it is an indirect measure, i.e., one with low "transparency," it seems fitting to consider a paper which deals with this assumption. Cohn (14) cites evidence that the F scale correlates with intelligence and suggests that such a relationship may mean that more intelligent persons are able to penetrate the meaning of the F scale and thus respond in a more acceptable or "proper" way. To test these contentions he administered the F scale twice to a group of 64 college students. On the first administration they were instructed to respond as they thought "highly educated, intelligent people" would; on the second administration they were told to respond as they thought "unskilled laborers of low intelligence" would. The mean scores were 2.39 and 5.45, respectively, for the first and second administrations; this difference was significant at the .01 level. Cohn also correlated the difference in scores with the "Altus Verbal Aptitude Test" and found the relationship to be significant at the .01 level. It was Cohn's conclusion that ". . . the F scale can be faked and therefore cannot be considered an indirect measure. Furthermore, it would appear that intelligence is related to the ability

to 'fake good'" (14, p. 732).

In a similar vein, Cohn (16) has also suggested that it may be possible that the F scale measures authoritarian submission, because people who have a tendency to respond positively in an ambiguous situation are submissive, rather than because of item content. Leavitt *et al.* (38), taking this study as a starting point, have further investigated this aspect of the F scale. They reworded half of the original items negatively and administered the F scale (half positive and half negative items) to five independent populations. They found generally that with high scorers (based on the unchanged half) there were negative or low positive correlations between the two halves, while with low scorers the correlations between the two halves were fairly high and positive. This was taken to indicate that ". . . the present design of the F scale confounds form and content variables, but in the 'right' direction; that authoritarian people as measured by the scale agree more with authoritative statements; and that, therefore, a portion of the discriminatory power of the F scale derives from its form, rather than its content" (38, p. 221). A somewhat comparable finding has been reported by Mullery *et al.* (48).

Related to this last study, Bass (4) has investigated the hypothesis that one's ". . . performance on the F scale has less to do with the content validity of the items than with the response set to acquiesce to any generalizations about social issues—authoritarian or equalitarian" (4, p. 616). He composed a scale made up of opposites of each of the F-scale items. After having the new scale items rated by a group of students on their opposition to the original items, he constructed two scales each made up of half new items and half original items. These scales were admin-

istered to students in psychology classes.

The tendency for an individual to support both the new items and the original items was taken as a measure of a response set to acquiescence. It was found that there was a greater tendency toward acquiescence as the items became more ambivalent; a factor analysis indicated that about three-fourths of the reliable variance of the F scale could be accounted for in terms of this response set. Bass, therefore, concludes ". . . that a much more parsimonious explanation can be given to account for the positive relations between authoritarianism, misanthropy, xenophobia, and ethnocentrism, where all are measured by scores based on acceptance of generalizations about social affairs. It appears that these correlations may be due primarily to a response set to acquiesce to *any* generalization about social issues" (4, p. 623).

Webster, Sanford, and Freedman (63) have developed a 123-item scale based on the F scale which is said to tap the authoritarian syndrome; it correlates .74 with the F scale, while giving evidence of being harder to fake. Some 677 items drawn from various personality tests including the MMPI and the California Personality Inventory were used in its construction. This new scale is said to be less ideological and more personality-centered than the F scale.

#### SUMMARY AND CONCLUSIONS

Some sixty-odd studies involving the F scale (2), reported from 1950 through 1955, were surveyed. It was noted that scores derived from the scale have been used in studies of prejudice, leadership, rigidity, adjustment, and group behavior, among others. Furthermore, the F scale has been found to correlate with intelligence, xenophobia, family ideology,

anxiety, re-enlistment intent, and co-operation in experimentation, to name but a few of the multitude of variables to which it has been related.

Without entertaining a broad critique, one may nonetheless be awed by the massive and amorphous area which has been touched upon by F-scale researchers. To comprehend it requires a theoretical substructure which is not yet available, except in kaleidoscope form. But some distinguishable points do stand forth. Notable among these is the evident fact that the F scale correlates most systematically with other paper-and-pencil measures, and least systematically with interpersonal behaviors, particularly as situational conditions are varied.

In this regard, it is well to recall that intelligence and educational level have both been found to relate negatively to the scale. It is not astonishing, then, that a general intelligence factor may operate here to provide the basis for a good many of the *r*'s which have been so zealously garnered. That intelligence underlies some of these relationships, is only part of the story. There is evidence, too, that various response sets may account for still more of the variance involved.

Regarding interpersonal studies, it is clear that relationships found with "leadership," let us say, have not held as the situation was altered. Surely this ought not surprise anyone in light of the newer conceptions

of situational determinants. But even more significant, in this picture, is the concomitant inclination to impute a correspondence between ideology, personality, and institutional form so as to view them as having a congruency of "authoritarian" identity. This may be theoretically acceptable, but it is not empirically supportable, at this juncture. Only confusion has derived from this confounding of elements in research undertakings.

The more ultimate questions regarding the scale's place as a personality measure are not readily resolved by recourse to the works covered. Deeper consideration of this issue has been presented elsewhere (13). At this writing, one may say that, if anything, the F-scale literature begs questions about the very definition of such terms as "personality" and "syndrome" themselves.

Today, although much work has been done using the F scale, one might reasonably hesitate to recommend it as a "practical" instrument in applied settings. Many questions still remain to be answered before its use would have much meaning in terms of predicting human behavior. Perhaps the greatest need, if the F scale is to be used still more in research, is for evidence regarding its behavioral implications. Beyond this, adequate studies of the scale *qua* scale are required; hopeful signs in this direction are to be seen in some of the work reported.

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## BRAIN INJURY AND MORPHINE WITHDRAWAL<sup>1</sup>

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It is difficult to assess fully the phenomenon of the disappearance of opiate withdrawal symptoms after prefrontal lobotomy, since it is of such recent note and so little has been reported on it in the literature. The conditions under which changes in the nature of narcotic addiction and withdrawal symptoms may be examined greatly restrict the availability of observable material. Patients must be (a) "addicted" to a drug for a sufficient time period; and (b) suitable for frontal lobotomy—this implies that previous measures for the relief of intractable pain (among which the use of drugs was one) have failed. Not until 1946 were lobotomies attempted specifically for the relief of pain (3, 4), and the changes in drug needs which the operations effected were noted incidentally or not at all. In 1952 the first controlled, experimental study was performed with specific regard for these changes, and to date it is the only one (25). It nevertheless bears comparison with previous reports as long as the differences in design are borne in mind.

In the field of treatment for narcotic addiction as a primary disorder rather than as secondary to the alleviation of pain, many pharmaceuticals have been employed to assist the withdrawal procedure. The synthetic analgesics (methadone, meperidine, etc.) have been used most often, but in 1941 Avery and Camp-

bell attempted to reduce withdrawal stress through the use of metrazol shock and carbon dioxide inhalations (1). The results were not particularly encouraging, though they led to the later utilization of electroshock in cases of both primary morphine addiction and intolerable pain (15, 20). More recently, attempts have been made to alleviate abstinence distress and to aid in the "cure" of primary addicts by the administration of adrenal cortex compounds and tropic hormones (ACE, cortisone, ACTH) and other substances having adesoxycorticosteronelikeaction (17), on the theoretical premise that drug addicts (and alcoholics too) suffer from "hypoadrenocorticism" or "a level of adrenal insufficiency comparable in certain respects to a sub-clinical Addisonian syndrome" (18).

The use of lobotomy for the relief of primary addiction might have been resorted to by neurosurgeons had it not been for the fact that most of them considered the postlobotomy syndrome to be a far more serious condition than drug addiction. However, in cases of intractable pain with secondary addiction arising as a medical complication, many felt the balance favored surgery, and it is from reports on cases of this nature that most of the present material is drawn. Discussion of the discrepancies obtaining among such reports will follow this section. An attempt will be made to derive meaning from them in terms of a theory of psychosurgical action with regard to addiction.

<sup>1</sup> This work is not to be construed as necessarily reflecting the views of the Department of the Navy.

### POSITIVE REPORTS: ABSENCE OF WITHDRAWAL SYMPTOMS AFTER LOBOTOMY

James L. Poppen gives a report of a patient with pain associated with the fifth cranial nerve (14). All other methods failed to provide relief. After bilateral prefrontal lobotomy there was a complete cessation of her complaints. "Not at any time was there a craving for morphine nor did she once ask for a hypodermic needle."

Mason and Hanby report an unusual type of persistent pain and an associated severe morphine "addiction" in a paraplegic patient which were relieved by bilateral prefrontal lobotomy (13). Prompt removal of the drug on the second postoperative day produced relatively few withdrawal symptoms. "It is suggested that the latter group of symptoms are largely psychic in nature."

Dynes and Poppen report on lobotomy for the relief of intractable pain performed on 18 nonpsychotic patients at the Lahey Clinic (3). Relief was accomplished and included loss of anxiety over impending death. Those patients who had been "addicted" to narcotics were able to dispense with them completely. Some patients, however, had severe withdrawal reactions which were in turn relieved by administration of narcotics for a brief period after which gradual withdrawal was easily carried out.

Koskoff, reporting on a series of patients subjected to unilateral lobotomy, found no significant symptoms due to drug withdrawal (9). Dynes and Poppen are skeptical, and claim that the unilateral procedure, as demonstrated in their series, afforded no relief from withdrawal stress.

Scarff supports Koskoff in his publication of results of the unilateral technique in 33 patients, reporting

the outcome as "good" in 66 per cent, "fair" in 18 per cent, and "poor" in 15 per cent (16). In 15 patients of the series having "careful psychometric evaluations" before and after lobotomy, no significant impairment of intellect or personality could be found. In 15 of 16 patients heavily "addicted" to narcotics because of pain prior to operation, abrupt termination of the narcotic was effected immediately after operation without withdrawal symptoms. The additional patient, appearing to have a primary narcotic addiction, was likewise cured of her dependence on the drug. These data, the author claims, indicate that unilateral prefrontal lobotomy is both an "effective" and an "acceptable" measure for the relief of intractable pain in many conditions where other measures cannot be applied. The value of the procedure in terminating drug addiction due to pain seems "well established." "The case cited gives promise that the operation may have value also in the termination of primary addiction."

### NEGATIVE REPORTS

In contradiction to the claims of Scarff is the assertion by Freeman and Watts that "chronic alcoholism and drug addiction [Scarff's "primary" addiction], when they are manifestations of underlying psychopathy or severe psychoneurosis, are contraindications for lobotomy. Even though the emotional tension may be relieved, the compulsion may remain and result in complete irresponsibility" (4). In their general report on the problem these workers note that after lobotomy their patients no longer requested medication (narcotics), so none was given. They soon discovered, however, that withdrawal symptoms could occur, so they con-

tinued treatment for several days following operation and then withdrew the drug when it no longer seemed necessary. They sum up:

In patients who have been receiving large doses of opiates, it would seem that the combination of postoperative confusion and inertia prevented the patient from appreciating the discomfort due to withholding the drug. He was likely, therefore, to experience severe physiologic disturbance. It is well to continue for several days the habitual dose of the narcotic, and then gradually to reduce the amount.

#### WIKLER'S POSITION

Wikler, in his experimental studies at the USPHS Hospital in Lexington, Kentucky, emphasizes that physical dependence is not synonymous with addiction (25). On this premise he distinguishes between the "purposive" and "non-purposive" aspects of addiction in an attempt to create an operational distinction. The former is the craving (psychological component), and the latter the nonsymbolic element (physiological component). With this in mind he concludes:

Bilateral frontal lobotomy reduced markedly the "purposive" features of the morphine abstinence syndrome, but it did not affect the "non-purposive" abstinence changes if abrupt withdrawal of morphine was delayed as little as one week after operation. When withdrawal was carried out coincidentally with lobotomy, the non-purposive changes were definitely attenuated, possibly because of the temporary reduction in reactivity of the autonomic nervous system due to "diaschisis."

The justification for this last assumption of Wikler's need not concern us here, though it may be well to point out that the "diaschisis" effect is of controversial significance.

What is different about Wikler's study is that the lobotomies were performed on schizophrenics, for relief of the psychosis, not for the alleviation of pain, and a pharmacological "addiction" was experi-

mentally induced purely for purposes of research. This afforded an opportunity to study primary addiction (though in psychotics), without the complicating variable of a preoperative "addictive personality" to contend with. At the same time, this difference allows one to be fairly certain that the cessation of craving for drugs is a direct result of the operation rather than of the termination of pain with accompanying loss of the need to allay it.

Another feature of the work at Lexington is the emphasis on the temporal component. Careful regard for this factor enables Wikler to clarify what is probably the cause of most of the controversy in the literature, namely, the failure to observe and keep uniform the exact time at which withdrawal is initiated. Wikler demonstrates rather conclusively that a difference of one week can completely alter the results, or even reverse them.

#### THEORETICAL FORMULATIONS

##### *Physiologic Basis of "Addiction"*

The phenomenon of the disappearance of opiate addiction residuals following frontal lobotomy is of considerable intrinsic interest. But what is interesting as a phenomenon gains scientific relevance only if it can be related to theory. In the present instance the neurophysiological mechanisms deserve explanation. But these mechanisms are obscure; only their effects are known. It appears established, if we follow Wikler, that the integrity of the reciprocal pathways from the thalamus to the superior and lateral areas of the anterior frontal cortex and to the rostral orbital cortex is essential for the mechanisms which subserve the purposive components of the abstinence

syndrome but not for those which subserve its nonpurposive components (22, 24). He warns, however, that this observation cannot be cited as evidence of either "organogenesis" or "psychogenesis" in the development of these features of the syndrome. "While consideration of the subjective and physiologic aspects of these phenomena may be useful in understanding and in treating addicted patients, inferences regarding etiology which are based on the reaction pattern alone [reduction of craving by lobotomy] are heuristically unwarranted" (25).

It is known, therefore, that certain anatomical connections are necessary to integral psychological aspects of drug addiction, but it is not known why this is so. In the "essential" character of these connections lies the reason for the changes observed after lobotomy, but calling them essential does not provide an explanation for the addiction phenomenon itself, unless it is assumed that the difference between lobotomy and morphinization is one of degree rather than of kind, in which case the stronger, more inclusive effect of the former subsumes the weaker effect of the latter and so eliminates the special need for it.

According to Wikler, the potent analgesics all depress after discharge mediated through closed internuncial chains without affecting other responses in the central nervous system. With appropriate techniques he has demonstrated that "biologic dependence of the organism on morphine develops at spinal, diencephalic and cortical levels of integration, and that the processes involved appear to be adaptive responses to the selective depressant effects of the drug" (23). The experimental data indicate, to Wikler, that the genesis of

both the "purposive" and the "nonpurposive" components of the abstinence syndrome is related, at least in part, to these changes in the organism and that the changes are independent of symbolic significance (26). He admits, though, that, theoretically, they "may become 'conditioned' to meaningful stimuli, and there is some clinical evidence that this occurs" (23). Such evidence is the starting point for the formulations of Lindesmith (10) who feels that an adequate theory of opiate addiction can be developed which is based on the facts of conditioning alone. His clinical data are not conclusive, however, and are at variance with those of most experimental work, which tend to show that the problem is too complex for such a simple solution, though, of course, the objections raised are not based on any opposition to theoretical parsimony.

#### *Pain*

When it was discovered that pain could be relieved by lobotomy and similar procedures, workers made immediate use of the new techniques. Investigators attempted to explain not only how surgery and electric shocks could "cure" mental disorders but also how these techniques could relieve intense physical suffering without any direct interference with the pathways presumed to mediate pain. The theoretical rationales offered for the therapeutic effect of shock treatments became so numerous as to be almost worthless. Gordon (5) wrote an article listing fifty different (if somewhat overlapping) formulations, and since then more have appeared. Formulations purporting to account for the results of psychosurgery are fewer in number, and in general seem to converge

on a single, fairly well circumscribed point. It has been noted that the operation tends to produce in most patients an essentially similar result: a lackadaisical attitude with reduced motivation and a more spontaneous, less inhibited affect. It seems to "release" constriction and compulsion and to reduce what Freeman and Watts called "psychic pain." The reporting of these effects threw a spotlight on the need for a new definition of pain. It was no longer tenable to hold that pain was a purely sensory experience. It was apparent that more was involved in the complaints of patients than perception of pain alone, inasmuch as the complaining usually ceased postoperatively, nor were these patients "bothered" by what had been, preoperatively, excruciating agony. Yet, upon questioning, nearly all admitted that the pain was still present, unabated, in spite of the fact that they were able, for the most part, to ignore it. This puzzling clinical fact led Freeman and others to speak of "suffering" rather than of pain and to hold that there was considerable cortical modification involved in the subjective reaction to most types of pain. Freeman theorized: "Lobotomy prevents the continuous contemplation of pain . . . It becomes of momentary importance . . . The emotional aspects of the pain phenomenon are done away with." MacKay (11) states: "The interference with pain experience that results from lobotomy or cortical ablation consists largely of the abolition of affect. This is consistent with what is known about the phylogenesis of the nervous system." Keats and Beecher (8) assert that: "The pain experience of man consists of both perception of painful stimuli (inflicted, experimental pain) and the psychic modifi-

cation of these stimuli (as in existing pain)." They present an hypothesis to explain the analgesic properties of phenobarbital by depression of the internuncial spread of pain impulses in the brain and inhibition of the "psychic" phase of pain experience. An analogy to prefrontal lobotomy is drawn. Their argument is in line with the results of the experimental work of Robert Malmo (12). With the Hardy-Wolff-Goodell pain threshold apparatus (7), which uses radiant heat as the stimulus, he established that postlobotomy patients "show increased responsiveness to external stimulation." Chapman *et al.* confirm this. They write that "patients may be relieved by lobotomy of intractable pain and yet seem to tolerate less readily stimuli such as are applied to the skin" (2). Thus the emotional component, i.e., the subjective significance of the chronic pain experience, is considered by these authors to be of primary importance in their suffering.

#### *Denial*

More than the mediation of emotions is attributed to the integrity of the fronto-thalamic projections. "Motivation" is also considered to be a function of the areas connected by these pathways, and reduction of motivation is used as an explanatory principle for the appraisal of the effects of lobotomy on patients with mental disease. Wikler (25) uses it also as one way of accounting for the reduction of the purposive features of the abstinence syndrome after lobotomy, saying that it may be part of the general reduction of motivation resulting from the operation. However, he also notes an alternative explanation based on the concept of "anosognosia" (denial of disease). The patient suffering from some kind

of cerebral damage with anosognosia denies obvious symptoms of his illness; the subject who is euphoric denies obvious, realistic sources of anxiety. In both instances there is evidence of generalized impairment of brain function. It has been suggested that anosognosia represents a mechanism that the individual with adequate brain function normally utilizes to some extent but which, in the brain-damaged patient, is "enduring and rigid" (19). However, to validate this for his series, Wikler points out, it would be necessary to demonstrate that the same changes occurred in patients with comparable damage to parts of the brain other than the frontal lobes. It has also been suggested that individuals who have cultivated psychological defenses against anxiety other than denial are less apt to exhibit marked disintegration of the personality under the stress of abrupt withdrawal of morphine, as well as from the effects of brain damage. Such individuals have other resources on which to fall back, so that loss of the drug which facilitated the use of the denial mechanisms is less traumatic.

Weinstein, Linn, and Kahn (20) sum up their discussion of the condition of anosognosia in relation to electroconvulsive therapy for intractable pain by stating:

In the stage of greatest alteration of function, the patient was euphoric and denied her pain; with a lesser alteration of function she was paranoid and showed a partial denial, while with the least alteration of function [least cerebral modification accruing to the electroshock], she was depressed and completely aware of pain.

It is suggested that the therapeutic efficacy of electro-convulsive therapy in functional depressions derives from the production of a state of brain function in which the mechanism of denial is facilitated in characterologically disposed individuals.

This "mechanism of denial" is an

old conception, clinically. Freud used it in a psychiatric context to describe an ego-defense technique. Babinski created the term "anosognosia," but claimed that only right postparietal lesions would produce the syndrome. Regardless of the frame of reference, however, in which the term originated, it has seemed justifiable to extend its application to all cases in which there is an attitude of denial in the face of threatening reality. Guthrie and Grossman (6) conceive of the syndrome as "an adaptation to stress found in persons who are facing internal disorganization and an inability to cope with the demands of the environment," while the various phenomena of denial are conceived of by Weinstein and Kahn as manifestations of a defensive psychological attitude adopted by some patients with gross structural defects of the central nervous system as a means of immediate protection against the recognition of life-threatening disease processes. They include denial of many defects of function and of events, such as operations, in the patient's recent experience. According to these authors, the premorbid personality is characterized chiefly by perfectionistic trends. This may be somewhat gratuitous on the basis of their present evidence, though other writers, particularly those dealing with psychosurgical techniques, have indicated that those patients who have the best prognosis are obsessive-compulsive types.

The facilitation of denial, then, provides a possible "explanation" for the reduction of the purposive features of the morphine abstinence syndrome by bilateral prefrontal lobotomy. It also provides a rationale for the phenomenon of reduction of intractable pain (i.e., reduction of the interpretation of pain as

intolerable) and a reason, alternative to "diaschisis," for the reduced stress of the nonpurposive aspects of morphine withdrawal. The success of other procedures in facilitating denial remains to be tested, and study of the problems represented by anosognosia and denial in a variety of experimental designs seems called for. Results of studies in this area would have application to many clinical neurologic and psychologic situations besides the addiction-lobootomy relationship.

Weinstein and Kahn have noted a tendency for many patients who are receiving electroshock therapy to become "addicted" to the treatment (21). They display an eager anticipation of each new shock application and complain if it is not forthcoming. This observation points up a possible neurophysiological parallel between the effects of morphine or other addictive drugs and an electrically induced impairment of brain function. The datum lends itself to the interpretation of a psychological relationship in terms of the denial mechanism. It remains for research to determine to what extent a purely pharmacogenic addiction may be compared with a nonpharmacogenic addiction in terms other than the verbalizations of patients. Experiments with animals, in which the symbolic factors might be excluded, should be helpful here.

#### SUMMARY

A review of the literature indicates lack of agreement as to the presence or absence of opiate withdrawal symptoms following surgically im-

posed brain damage. The controversy is resolved when recourse is made to the temporal factor, i.e., the time at which withdrawal is carried out relative to the time an operation is performed. It is suggested that the withdrawal phenomenon is as complex as addiction itself, and that both are related to many peculiar effects that accrue to cerebral tissue destruction. An hypothesis is advanced which purports to account for the loss of craving for drugs, the reduction of abstinence distress, and the relief of intractable pain following frontal lobotomy, on the premise that damage to the brain areas involved facilitates the employment by the patient of the mechanism of denial, a psychological potential present in everyone and ready for use to a greater or lesser degree depending upon both the individual personality make-up and the situation which calls it out. In brain "damage," whether caused by morphine addiction, electric shock or lobotomy, denial is exaggerated and may, in fact, become the sole psychological crutch in persons so disposed. To the extent that various pharmacological or surgical states can induce either temporary or permanent (functional or structural) cerebral "damage," they may substitute for one another (within the limits of their positions in the hierarchy of comparative destructiveness) in acting as the facilitating agent for the use of denial of whatever physiological or psychological stress situation might be present. Apparently, the greater the damage the more complete the facilitation.

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## THE INTERVAL ESTIMATION OF A TRUE SCORE

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Because of the measurement error in psychological assessment, an obtained score,  $X$ , is commonly viewed as an *estimate* of the respondent's true score,  $X_t$ . As a result, a number of writers, e.g., Garrett (1), Guilford (2), Gulliksen (3), Johnson (4), and Thorndike and Hagen (5), among others, have emphasized the practical importance of attaching to  $X$  an *interval* about this obtained score, together with a *level of confidence* that this interval about  $X$  contains  $X_t$ .

The customary technique, e.g., (1, p. 343), (2, p. 352), (3, p. 20), (4, p. 117), and (5, p. 134), assumes that the respondent's true score,  $X_t$ , is the mean in a normal population of obtained scores,  $X$ —the population being the conceptual result of parallel, i.e., equivalent, retesting of the respondent. It is further assumed that the standard deviation,  $s_x$ , of this distribution of  $X$  is the standard error of measurement  $s\sqrt{1-r}$  where  $s$  is the test standard deviation and  $r$  the test reliability, computed for a stably large and representative norm group chosen for its relevance to the respondent at hand. The technique then concludes, e.g., that

$$X \pm 1.96 s\sqrt{1-r} \quad [1]$$

contains  $X_t$  with 95% probability. Thus, for a particular norm group, given that the reliability coefficient of an aptitude test in mathematics is .92 and the standard deviation is 15, Garrett (1) states that if a particular subject has "a score of 85, we may feel confident (the chances are .95) that his [true] score 'actually' lies

between 77 and 93," (i.e.,  $X_t$  lies between  $85 \pm (1.96)(15)\sqrt{1-.92}$ ).

However, although the customary technique assumes  $s_x = s\sqrt{1-r}$ , it follows directly from their formal definitions (6, p. 292) that, in fact, the  $s_x$  of a particular respondent and the standard error of measurement  $s\sqrt{1-r}$ , of a relevant norm group are, in general, unequal. An intuitively clear demonstration springs from the fact that the bare possibility of computing an  $s_x$  arises only if the subject has been tested and once retested, so that at least two values of  $X$  are available. Since in practice only a single  $X$  is ordinarily available, usually  $s_x$  is not known; and is, in fact, not even computable.

Nevertheless, it will now be noted that *the customary technique is satisfactory if, for the norm group of reference, r is not low and if the obtained score X of the subject concerned is not an extreme deviate from the mean, X, of the norm group.*

Assume that to a first approximation, the regression, over the norm group, of true score  $X_t$  on obtained score  $X$  is linear (3, p. 43). It then follows directly<sup>1</sup> that an interval regression estimate of  $X_t$  is, e.g., at the 95% level of confidence,

<sup>1</sup> In general, if a parameter has an unbiased point estimate which is normally distributed, then an interval estimate, say at the 95% level, of the parameter is: (point estimate)  $\pm (1.96)(\text{standard error of the point estimate})$ .

In the present case, the parameter is  $X_t$  and the point estimate, based on least squares linear regression, is  $Xr + \bar{X}(1-r)$  with standard error of estimate  $s\sqrt{1-r}\sqrt{r}$  (3, p. 43).

$$[Xr + \bar{X}(1-r)] \pm 1.96 s\sqrt{1-r}\sqrt{r}. \quad [2]$$

Thus, in the above example, if  $\bar{X}$  were 100, the 95% interval for  $X_t$  is 79 to 94 (i.e., with 95% confidence  $X_t$  lies between  $[(85)(.92) + (100)(1 - .92)] \pm (1.96)(15)\sqrt{1-.92}\sqrt{.92}$ ); this is a negligible discrepancy with the interval 77 to 93, obtained above by the customary technique.

The preceding italicized rule about the customary technique being satisfactory for high  $r$ , and for  $X$  in the vicinity of  $\bar{X}$  follows at once upon contrasting Equations 1 and 2.

Fortunately, Equation 2 furnishes an interval estimate for  $X_t$  in case the customary technique fails, i.e., in case  $r$  is only moderately high and/or  $X$  is an extreme deviate. The only

information needed in addition to the  $r$  and  $s$  norms, required by the customary technique, is the norm  $\bar{X}$ . All three norms,  $r$ ,  $s$ ,  $\bar{X}$ , should be based, of course, upon the norm group most relevant to the subject at hand.

Why does the customary technique, 1, work as well as it does, in view of the fact that  $s\sqrt{1-r}$ , is not equal to  $s_s$ ? A partial explanation is the fact, flowing directly from their definitions, that although the standard error of measurement,  $s\sqrt{1-r}$ , is not in general equal to a particular  $s_s$ , it is equal to the *average* of all the  $s_s$ s of the members of the norm group. Hence the customary technique, 1, is based on the replacement of a particular unknown  $s_s$  by an average  $s_s$ .

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## THE RETICULAR FORMATION AND BEHAVIORAL WAKEFULNESS

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In a paper considering the relationship of brain waves to behavior, Ellingson (2) concluded that the brain stem reticular formation (BSRF) is identical with Kleitman's "waking center" (4). The evidence Ellingson cites is consistent with such an interpretation. He states "It is clear from these results that the BSRF is essential to the maintenance of the waking state under normal conditions . . ."; and, "Taken together these findings indicate that a background of maintained activity in the BSRF accounts for the maintenance of wakefulness, while reduction of its activity precipitates a state of somnolence or unconsciousness." These quotations indicate the author's belief that level of activity of the BSRF is a necessary and sufficient condition for behavioral wakefulness. However, various observations do not support the notion that reduction of activity of the reticular formation must result in behavioral sleep.

Funderburk and Case showed that atropine induces changes in the EEG pattern which are characteristic of sleep in monkeys and cats (3). When

a sufficient amount of atropine had been given to an animal, arousal of the "alerting reaction" (EEG) was difficult even though behaviorally the animal was awake. Similarly, Wikler observed that atropine and N-allylnormorphine gave rise to the EEG sleep pattern but did not produce sleep in the dog (6). Bradley and Elkes obtained this same result with L-hyoscyamine and atropine in the cat (1). Rinaldi and Himwich found that atropine could in fact completely block activity of the reticular formation in an otherwise alert rabbit (5). Thus, behavioral wakefulness can accompany reduction of activity in the BSRF.

The pharmacological data cited here indicate inactivity of the BSRF by itself is an insufficient condition for the occurrence of sleep. Consequently, sleep and the so-called "sleep pattern" are not necessarily correlated. However, these comments do not indicate that depression of the BSRF is not involved in the sleep process. Sufficient activation of the BSRF may prohibit behavioral sleep.

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## COMMENTS ON SCHMIDT'S "THE RETICULAR FORMATION AND BEHAVIORAL WAKEFULNESS"

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Before discussing Schmidt's comments on the role of the reticular formation in wakefulness (7), I would make it clear that I do not hold that any of my statements have absolute or permanent validity. Professor Magoun himself has questioned whether the states of wakefulness and sleep are always necessary consequences of activity or lack of activity in the reticular system, and Lindsley (personal communication) points out that "there is much to be learned about the whole mechanism and no doubt the picture . . . will have to be modified in some respects as more information is uncovered."

Schmidt raises two interesting and related questions. First, is a slow-wave-spindle-burst pattern in the EEG *always* accompanied by sleep? Second, are wakefulness and sleep *always* consequences of activity in the reticular system?

Examination of the reports of Funderburk and Case (3) and Bradley and Elkes (1) fails to reveal clearly stated conclusions concerning the dissociation of slow-wave-spindle-burst activity in the EEG on the one hand and sleep on the other. However, since Wikler (8), unlike Funderburk and Case and Bradley and Elkes, directly attacked the first question in point, and since his results are quite clear, I will not consider the interpretation of the latter's observations here. Wikler concluded that "the mechanisms which subserve 'sleep' and those which subserve the 'burst-slow wave' patterns are distinct from each other, although

they are very often closely interlocked." This is not in disagreement with my indication (2) of a "close relationship between particular EEG patterns and the various stages of the sleep-wakefulness cycle." However a word of caution must be introduced. While the EEG samples published by Wikler from animals under atropine and N-allylnormorphine certainly greatly resemble those seen in natural sleep, it must be remembered that EEG pattern analysis is rudimentary (in fact largely impressionistic), and it is not possible to say that two similar-appearing patterns are identical.

Granting that sleep is dissociable from EEG "sleep patterns" under certain specifiable experimental conditions, brings us to the principal question raised by Schmidt, namely, are wakefulness and sleep always necessary consequences of activity or lack of activity in the reticular system? The crucial experiments would appear to be those of Rinaldi and Himwich (4), who, according to Schmidt, "found that atropine could in fact completely block activity of the reticular formation in an otherwise alert rabbit." Whether or not their observations are conclusive depends upon the validity of both parts of this statement.

First, were their rabbits alert or awake under the conditions under which "complete blocking" of the reticular formation was demonstrated? In the paper cited by Schmidt (4) and in two related papers (5, 6) I can find no statement that Rinaldi and

Himwich observed alertness in their curarized atropinized rabbits. It is difficult, often impossible, to judge the state of consciousness of a curarized animal. However, Schmidt informs me (personal communication) that the investigators have observed rabbits to be behaviorally alert under the doses of atropine used, although this was not reported.

With respect to reticular blockage, Rinaldi and Himwich's studies did not *necessarily* demonstrate that atropine can completely block activity of the reticular formation. What they did demonstrate was that atropine can block the cortical arousal response which ordinarily accompanies reticular stimulation. They assume that this blocking occurs in the reticular formation itself, which is perhaps the most reasonable assumption, but it could occur instead, or more likely also, in the cortex or in some intervening structures.<sup>1</sup> In the studies cited Rinaldi and Himwich did not report monitoring the activity of the reticular formation itself.

In an earlier draft of these "comments" I suggested an experiment to clarify further the question of the role of the reticular formation: using noncurarized atropinized animals with recording electrodes implanted in the forebrain and the reticular formation, observations should simul-

taneously be made of overt behavior in an open field situation and of the respective electrographic tracings under conditions of no stimulation, peripheral stimulation, and direct reticular stimulation. Schmidt informs me (personal communication) that Rinaldi and Himwich "have done substantially the experiment you recommend and obtain the same EEG phenomena reported with rabbits which are behaviorally awake." When those results are published and confirmed the question should be greatly clarified.

The results of Rinaldi and Himwich of course do not show that depression of the reticular formation is not involved in sleep, as Schmidt has pointed out, or that reticular activity is not involved in the initiation and maintenance of wakefulness, as the repeatedly confirmed studies of Lindsley, Magoun, and their colleagues have so clearly shown. They do seem to indicate that other structures are involved in a parallel or similar function. The concept of brain "centers" for particular functions has rightly given way to a concept of dynamic brain systems. I initially introduced the term "wakefulness center" in connection with the historical reference to Kleitman's work, but perhaps it was unwise to retain it thereafter, even while substituting the article "a" for "the." As I think my discussion shows, I did not intend to give the impression that the reticular formation is by itself responsible for the state of consciousness.

In the light of the considerations raised by Schmidt, I shall have to retract the first sentence he quotes from my paper. The remainder of the section on sleep and wakefulness, however, stands as of this date, with the addendum that other structures may play roles similar to that of the reticular formation.

<sup>1</sup> In another paper (6) Rinaldi and Himwich present evidence that the locus of the atropine effect is not cortical. The evidence involves the failure of atropine to change the random slow activity following in the wake of acute isolation of one cerebral hemisphere from the rest of the brain. This evidence seems inconclusive with respect to the question under consideration here, since the random slow ("delta") activity associated with such traumatic, as well as with vascular and neoplastic, lesions is probably of different nature from the slow waves of sleep and would not be expected to vary with levels of consciousness or show electrical activation responses.

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## A SIMPLIFICATION OF THE SIGN TEST

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During the past few years, the popularity of nonparametric statistical methods has greatly increased. The sign test, because of its simplicity and efficiency, has become one of the most frequently used tests of significance. This test is most often used by psychologists to compare the effects of two different experimental conditions in situations where parametric tests are inappropriate. According to Dixon and Massey, it is most useful when ". . . a) There are pairs of observations on two things being compared; b) Each of the two observations of a given pair was made under similar conditions, and c) The different pairs were observed under different conditions" (1, p. 247).

When making comparisons between two methods, it is customary to select one method arbitrarily, scoring a plus for each comparison in which that method is higher, and a minus where it is lower. In theory, if the two methods are equivalent, the population would contain an equal number of pluses and minuses. The deviation of the sample from this expectancy is tested for significance.

For samples of less than 25, this test is ordinarily made by reference to a binomial table. For sample sizes larger than 25, a normal approximation or its equivalent chi square with one degree of freedom have both been suggested (2, p. 431).

However, for the case  $\alpha = .05$  and  $N \geq 25$ , there is an even simpler test of significance which requires no tables. In addition, this method pro-

vides a way of determining what sample size will be needed to discover significance at the .05 level for any anticipated disparity between pluses and minuses. The derivation is by simple algebra:

Let  $N$  = the sample size

$T$  = the obtained number of pluses

$P = Q = .5$  = the probability of a plus or minus under the hypothesis of no difference

$D$  = the difference between the proportion of pluses and minuses in a sample

$$D = \frac{T}{N} - \frac{N-T}{N} = \frac{2T}{N} - 1.$$

For samples with  $N \geq 25$ ,  $T$  is distributed approximately normally with mean =  $NP$  and standard deviation =  $\sqrt{NPQ}$  so that

$$\text{Prob} (|T - NP| \geq 1.96\sqrt{NPQ}) = .05$$

Since  $P = Q = .5$ ,

$$\begin{aligned} \text{Prob} \left( \left| T - \frac{N}{2} \right| \geq 1.96\sqrt{\frac{N}{4}} \right) &= .05 \\ \geq 2\sqrt{\frac{N}{4}} - .04\sqrt{\frac{N}{4}} &= .05 \end{aligned}$$

$$\text{Prob} \left( \left| T - \frac{N}{2} \right| \geq \sqrt{N} - \epsilon \right) = .05,$$

$$\text{where } \epsilon = .02\sqrt{N}.$$

Since  $\epsilon$  is less than 1 for samples with  $N \leq 2500$ , it can be ignored. Thus, we have:

$$\begin{aligned}
 & \text{Prob} \left( T - \frac{N}{2} \geq \sqrt{N} \right) \\
 & \quad + \text{Prob} \left( \frac{N}{2} - T \geq \sqrt{N} \right) = .05. \\
 & \text{Prob} \left( T \geq \frac{N}{2} + \sqrt{N} \right) \\
 & \quad + \text{Prob} \left( T \leq \frac{N}{2} - \sqrt{N} \right) = .05 \\
 & \text{Prob} \left( D^2 \geq \frac{2}{\sqrt{N}} \right) \\
 & \quad + \text{Prob} \left( D^2 \leq -\frac{2}{\sqrt{N}} \right) = .05 \\
 & \text{Prob} \left( D^2 \geq \frac{4}{N} \right) = .05
 \end{aligned}$$

which provides a simple test of significance. Thus, whenever  $D^2 \geq 4/N$ , the difference is significant at the .05 level. To illustrate, consider a finding

of 50 pluses and 25 minuses. Since

$$D^2 = \left( \frac{50-25}{75} \right)^2 = .11 > \frac{4}{75},$$

the difference is significant.

The fact that  $N \geq 4/D^2$  is also useful. It enables an experimenter to determine the sample size needed to discover significance if he suspects a given disparity. For example, suppose an experimenter is not concerned with demonstrating a difference unless there is a 3 to 2 ratio in either direction. He wishes to find the smallest sample size which will suffice. By solving

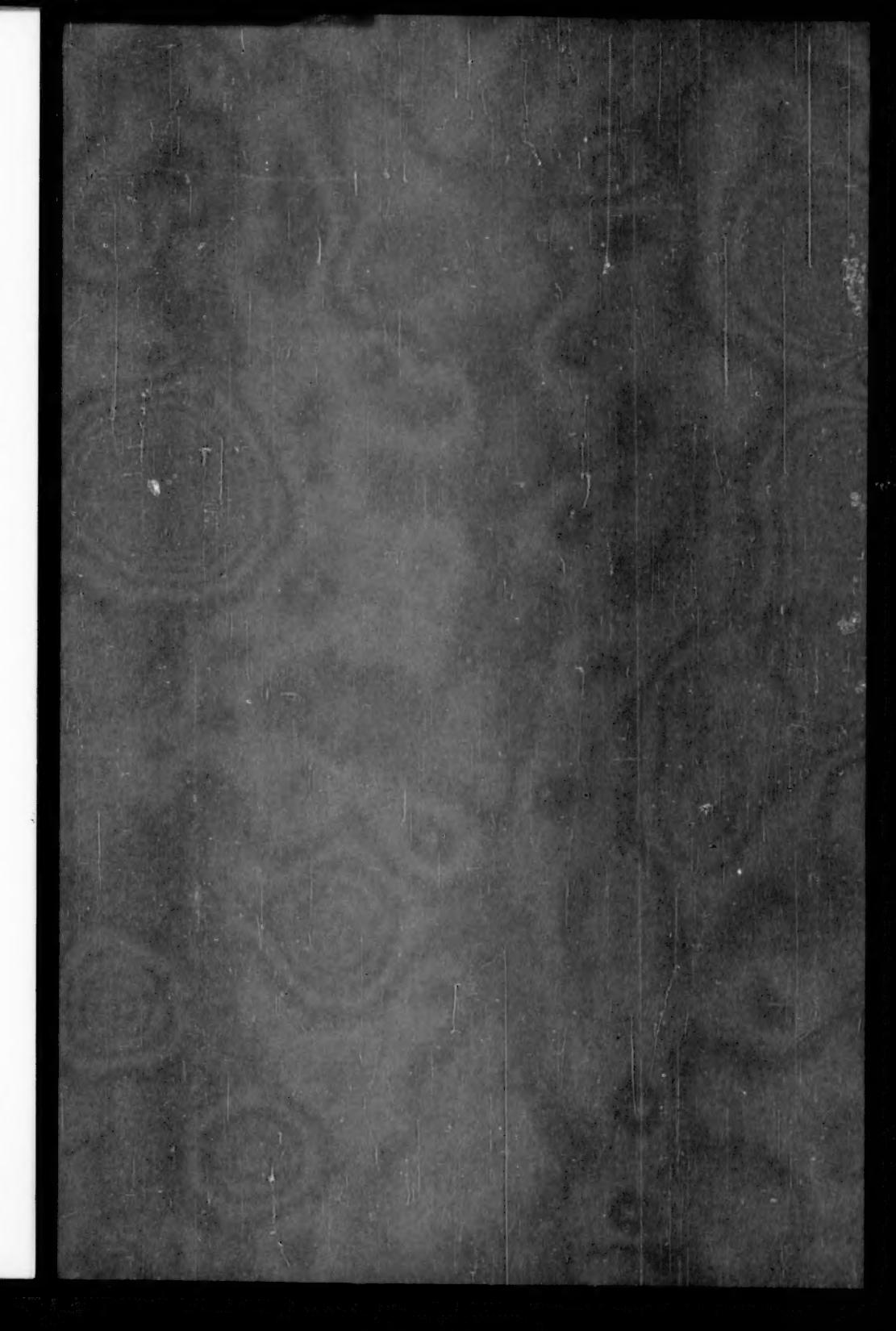
$$N \geq \frac{4}{D^2} = \frac{4}{(.6-.4)^2} = 100$$

he learns that he must make at least 100 comparisons.

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